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FRANCIS HEMMING, C.M.G., C.B.E.

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Colloquium on Zoological Nomenclature, Copenhagen, July-August 1953

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FOREWORD

The present is the first of the two volumes of the *Bulletin of Zoological Nomenclature* which were allotted to the publication of the Documents which had been gathered together to form the Agenda of the Colloquium on Zoological Nomenclature which it had been arranged should open at Copenhagen on 29th July 1953.

2. The Agenda for the Colloquium comprised seventy items, of which the first seven were concerned with problems which the Thirteenth International Congress of Zoology, Paris, 1948, had deferred for further consultations with a view to definitive decisions being taken by the next (Fourteenth) International Congress of Zoology when it should meet at Copenhagen in 1953. These problems were the subject of special review in a series of papers published in February and March 1952 in a volume—volume 7—of the *Bulletin of Zoological Nomenclature*, in which an appeal was made to interested specialists to furnish statements of their views. The response to this appeal was very gratifying, the total number of communications received in regard to these seven cases (including a small number of documents received before the publication of the foregoing appeal) amounting to one hundred and thirty-five.

3. The total number of documents which it was decided to lay before the Colloquium amounted to three hundred and seventy-one and the task of sorting, and arranging for the printing, of so large a mass of documentation was considerable. It was evident by the beginning of 1953 that a major effort would be needed if the whole of this documentation was to be published in time for it to be considered by the Colloquium when it met at Copenhagen some six months later. In order to expedite the publication of these documents, it was accordingly decided to publish the documentation for the Agenda in two volumes of the *Bulletin of Zoological Nomenclature*, instead of in one volume, as previously contemplated, and to arrange for these two volumes to be printed concurrently by different printers. Hence, it was that the Agenda for the Colloquium was published in volumes 8 and 10 of the *Bulletin*. (It was not possible to arrange for these volumes to be given consecutive numbers, for, after volume 8 had been allocated to the publication of the Agenda but before the decision to print the Agenda in two volumes, a start had already been made with the publication of volume 9, a volume devoted to the publication of applications on individual nomenclatorial problems submitted to the Commission for decision.)

4. For the reasons explained in the immediately preceding paragraph the present volume and its companion volume, volume 10, were produced under conditions of great pressure, owing to the rigid time-table necessary in order to secure publication before the opening of the Colloquium at Copenhagen in July 1953, a time-table which made it necessary to pilot through the press

some eight hundred pages of print within a period of about five months. It was unfortunately impossible in these circumstances to devote the normal amount of time to the reading of proofs and, in consequence, the present volume contains a considerable number of misprints, especially in papers either published in some language other than English or containing words in Greek or other languages using alphabets other than the Latin alphabet. The Trust offers its apologies to the authors of the papers concerned. A few other misprints are corrected on page 312.

5. The present volume contains the extensive documentation relating to the first three items on the Agenda for the Colloquium. Each of these items was concerned with one of the major problems deferred by the Paris (1948) Congress for consideration at Copenhagen in 1953. These problems were : (1) the problem of securing greater stability in zoological nomenclature (Case No. 1) ; (2) the question whether " neotypes " should be recognised in the *Règles* as a category of type specimen (Case No. 2) ; (3) measures required for the clarification and completion of the provisions in the *Règles* relating to the naming of Families (including Superfamilies) and lower taxonomic categories above the genus-level (Case No. 3).

6. It will be appreciated that the documents published in this volume represent only the working papers which were before the Colloquium when it considered the various nomenclatorial issues concerned and that in numerous cases the opportunities for interchanges of views presented by the long discussions which took place at the meetings of the Colloquium led individual specialists who had presented documents published in the present volume to modify their views in the light of considerations advanced by other members of the Colloquium. For these reasons and because in the case of each of the problems concerned a definite decision was reached by the Copenhagen Congress, the interest of the present volume is mainly historical. The documents published in the present volume should therefore be looked upon only as being in the nature of a background to the decisions taken by the Copenhagen Congress and should be read only in conjunction with those decisions.

7. The decisions on the questions dealt with in the present volume taken by the Fourteenth International Congress of Zoology, Copenhagen, 1953, on the advice of the International Commission on Zoological Nomenclature, assisted by the Colloquium on Zoological Nomenclature, together with decisions taken by it on other questions of zoological nomenclature, have been published, at the request of the Copenhagen Congress, by the International Trust for Zoological Nomenclature in a book entitled *Copenhagen Decisions on Zoological Nomenclature*. This book, which was published on 31st December 1953, is obtainable from : The International Trust for Zoological Nomenclature, Publications Office, 41 Queen's Gate, London, S.W.7 (price five shillings (5/-), plus postage).

8. The following are the references to the pages in the book *Copenhagen Decisions on Zoological Nomenclature*, where will be found the decisions taken

by the Copenhagen Congress in regard to the three major nomenclatorial problems, the documentation relating to which has been published in the present volume :—

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1	The problem of securing greater stability in zoological nomenclature	22–27
2	The question whether "neotypes" should be recognised in the *Règles* as a category of type specimen	28–31
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9. In completing the present volume the International Trust for Zoological Nomenclature desires to express its grateful thanks to Professor R. Spärck, President of the Fourteenth International Congress of Zoology for the admirable arrangements made by him for the accommodation of the Colloquium on Zoological Nomenclature during its meeting at Copenhagen, arrangements which contributed to an important degree to the successful outcome of its deliberations.

FRANCIS HEMMING

Secretary to the International Commission on Zoological Nomenclature

28 Park Village East,
Regent's Park,
London, N.W.1.

9th June, 1954.

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THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

PURCHASED
12 JUL 1953

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 8, Triple-Part 1-3 (pp. 1-108) 25th June 1953

ARRANGEMENTS MADE BY THE INTERNATIONAL TRUST FOR ZOOLOGICAL NOMENCLATURE FOR THE SUBMISSION OF MATERIAL FOR CONSIDERATION BY THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE AT ITS SESSION TO BE HELD AT COPENHAGEN IN 1953

By The Right Hon. WALTER ELLIOT, C.H., M.C., F.R.S., M.P.
(*Chairman, International Trust for Zoological Nomenclature*)

In order to promote the orderly consideration at Copenhagen of the problems of zoological nomenclature which will then be submitted to the International Commission on Zoological Nomenclature as a preliminary to being laid before the International Congress of Zoology, the International Trust for Zoological Nomenclature has arranged, on behalf of the International Commission, to devote two volumes of the *Bulletin of Zoological Nomenclature* to the publication of the documents which will then be considered by the International Commission. These volumes are the present volume (volume 8) and volume 10. The present volume will contain the first instalment of the material which has been received in response to the appeal made by the Secretary to the Commission (in volume 7) for assistance and advice in regard to the general problems which were remitted for consideration at Copenhagen by the Thirteenth International Congress of Zoology at its meeting held in Paris in 1948. The remainder of this material will be published in volume 10, which will comprise also the documents containing proposals in relation (*a*) to other aspects of zoological nomenclature and (*b*) to the work of the International Commission, which have been received from various sources.

2. Further, to facilitate the consideration at Copenhagen of the large mass of material submitted, the International Trust has convened a Colloquium on Zoological Nomenclature to meet in Copenhagen in the week preceding the opening of the Congress. It is hoped that by this means it will be possible to secure a sufficient measure of preliminary consideration of the numerous—and, in some cases, highly complex—problems involved to enable the International Commission to formulate generally supported proposals for submission to the Section on Nomenclature and the Congress itself when those bodies meet at Copenhagen. To this end invitations to the Colloquium have been issued to all institutions which are known to be interested in zoological nomenclature and to all individual specialists who have assisted in the preparation of material on nomenclature for consideration at the Copenhagen Congress.

ACTION TAKEN TO SECURE THE ADVICE AND ASSISTANCE OF INTERESTED INSTITUTIONS AND INDIVIDUAL SPECIALISTS ON THE ASPECTS OF ZOOLOGICAL NOMENCLATURE ON WHICH THE THIRTEENTH INTERNATIONAL CONGRESS OF ZOOLOGY, PARIS, 1948, INVITED THE SECRETARY TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE TO PREPARE REPORTS FOR CONSIDERATION BY THE FOURTEENTH INTERNATIONAL CONGRESS TO BE HELD AT COPENHAGEN IN 1953

By FRANCIS HEMMING

(*Secretary to the International Commission on Zoological Nomenclature*)

The purpose of the present note is to recall that the Thirteenth International Congress of Zoology, Paris, 1948, invited me, as Secretary to the International Commission on Zoological Nomenclature, to investigate certain aspects of zoological nomenclature, and to explain the action which I have taken under the foregoing mandate to obtain the views of interested institutions—and, in particular, nomenclature committees—and individual specialists on the action which it is desirable should be taken on each of these matters by the Fourteenth International Congress of Zoology when it assembles at Copenhagen in August 1953.

2. The problems which were so referred to me for investigation were the following :—

(1) The means to be devised for promoting stability in zoological nomenclature (Commission's file Z.N.(S.)359) ;

(2) Neotypes : the question whether this class of type specimen should be officially recognised in the *Règles* (Commission's reference Z.N.(S.)358) ;

(3) Family names and names of suprageneric categories of lower rank : measures needed for the clarification and amplification of the existing provisions in the *Règles* (Commission's reference Z.N.(S.) 357) ;

(4) Orders (and Sub-Orders) and Taxonomic Categories of Higher Rank : questions involved in the proposed introduction into the *Règles* of provisions regulating the naming of taxonomic units belonging to the foregoing categories (Commission's reference Z.N.(S.)360) ;

(5) Emendation of zoological names : proposed substitution for Article 19 of simple clear-cut rules capable of being easily applied (Commission's reference Z.N.(S.)356) ;

(6) Trivial names which, when first published, were treated partly as substitute names but which were also applied to particular species or specimens : treatment to be accorded in cases where the species so renamed is not the same as that to which the new name was also applied when first published (Commission's reference Z.N.(S.) 361) ;

(7) Names of nominal genera first published in generic synonymies : question of species to be accepted as the type species (Commission's reference Z.N.(S.)387).

3. In the period following the Paris Congress I took every opportunity which arose, either in correspondence or in personal discussion, of obtaining the views of zoologists and palaeonotologists as to the action which it was desirable should be taken on the foregoing matters by the Fourteenth International Congress of Zoology. In particular, with the assistance of the authorities of the Congress concerned, I read papers designed to elicit comments on the question of the further development of zoological nomenclature before the following bodies : (1) the Eighteenth International Congress of Geology, London, 1948 ; (2) the Congress of British Entomologists, London, 1950 ; (3) the Ninth International Congress of Entomology, Amsterdam, 1951. In the light of the views elicited in the foregoing manner I prepared at the end of 1951 papers on each of the questions which had been referred to me for study and report. In these papers I set out the issues which, as it seemed to me, arose for decision on each of the problems concerned and appealed to zoological and palaeonotological institutions and individual specialists to assist in the investigations asked for by the Thirteenth International Congress of Zoology by furnishing statements setting out their views as to the action which was desirable should be taken by the Fourteenth International Congress of Zoology at Copenhagen in 1953.

4. For the furtherance of the work of the Fourteenth International Congress, the International Trust for Zoological Nomenclature decided to devote a complete volume—volume 7—of the *Bulletin of Zoological Nomenclature* to the publication of the foregoing papers (and also of short notes on twenty-eight cases relating to individual names on which the Thirteenth Congress at Paris had asked for additional information). Volume 7 of the *Bulletin* was published in eight Parts in the period March-April 1952.

5. For the purpose of attracting the attention of interested institutions and specialists to the general problems discussed in volume 7 of the *Bulletin* (then in the press), a Notice was drawn up in February 1952 referring to these problems and soliciting the advice and assistance from all interested in these subjects. This Notice was published in the leading serial publications (such as *Science*) concerned with general biological problems, and was, in addition, despatched individually, under cover of letters inviting co-operation, to all known Nomenclature Committees, to the Natural History Museums and other

Scientific Institutions, members of the staffs of which were correspondents of the International Commission, and also to leading individual specialists known to the Commission as being interested in general problems of zoological nomenclature.

6. The material received in regard to the seven problems remitted at Paris for consideration at Copenhagen has been arranged in each case in date order. For convenience of discussion at Copenhagen, a " Case Number " has been allotted to each problem and a " Document Number " has been allotted to each document within each group. For example, the fifth document (measured by date of receipt) in regard to the second main problem bears the Document Number 2/5, and the tenth document of the fifth main group the Document Number 5/10.

7. The documentation received is being published forthwith, it being considered that this course is to be preferred to delaying publication until the Reports which I was asked by the Paris Congress to prepare on these subjects are available. These Reports will, however, be published as soon as possible in volume 10. It will be found that they consist of little more than summaries of the views expressed in the documents now published. It is hoped, however, that they will prove useful in view of the large number of the documents now published and the labour to the reader involved in obtaining a general conspectus of the advice received in regard to each of the large number of points discussed.

CASE NO. 1

DOCUMENTS RELEVANT TO THE CONSIDERATION OF THE PROBLEM OF SECURING GREATER STABILITY IN ZOOLOGICAL NOMENCLATURE

DOCUMENT 1/1

Origin of the present investigation

The record of the decision by the Thirteenth International Congress of Zoology, Paris, 1948, inviting the Secretary to the International Commission on Zoological Nomenclature to examine, in consultation with interested specialists, all means which might secure greater stability in zoological nomenclature and to submit a Report thereon was published in 1950, *Bull. zool. Nomencl.* **4** : 234.

DOCUMENT 1/2

A survey of possible means for promoting stability in zoological nomenclature prepared by FRANCIS HEMMING, Secretary to the International Commission on Zoological Nomenclature, in response to the invitation of the Thirteenth International Congress of Zoology, Paris, 1948, was published in 1952, *Bull. zool. Nomencl.* **7** : 148-188.

DOCUMENT 1/3

Professor PIERRE BONNET (Toulouse) presented a proposal for the incorporation into the *Règles* of a provision recognising a " Law of Prescription " to the International Commission on Zoological Nomenclature on 15th January 1948 for consideration during the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed to be inserted in the *Règles* was as follows :—

> Loi de Prescription : Tout nom qui sera resté ignoré durant de nombreuses années ne pourra, pour cause de priorité, remplacer un nom de genre ou d'espèce depuis longtemps utilisé ; s'il y avait lieu de le faire les specialistes en décideront.

For the full text of the foregoing application, see Bonnet, 1950, *Bull. zool. Nomencl.* **3** : 177.

DOCUMENT 1/4

The proposal by PIERRE BONNET (Toulouse) for the restriction, in the interest of stability in zoological nomenclature, of the Law of Homonymy as applied to generic names ; proposal presented to the International Commission on Zoological Nomenclature on 15th January 1948 for consideration during the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed was that the following new provisions should be inserted in the *Règles ;—*

> *Article* 34.—Homonymie : Tout nom générique n'est rejeté comme homonyme qui s'il a été utilise pour un autre genre dans un même grand groupe d'animaux ou dans un même pays : les grands groupes d'animaux sont : Protozoaires, Spongiaires, Coelentérés, Plathelminthes, Annélides, Némathelminthes, autres Vers, Crustacés, Myriapodes (*s.l.*), Insectes, Arachnides, Echinodermes, Procordés, Vertebrés.
>
> *Article* 34*bis*.—On ne reviendra pas sur les changements faites jusqu'en 1920 ; mais à partir de cette date seront maintenus contre le nom nouveau proposé tous les noms ayant déjâ une grande vogue et qui depuis de nombreuses années étaient employés sans la moindre confusion avec un homonyme méconnu d'un autre groupe.
>
> *Article* 34*ter*.—Cependant, à partir de 1940, il ne sera plus toléré qu'on utilise pour un nouveau genre un nom déjâ existant et se trouvant dans le *Nomenclator Zoologicus* de Neave (1939-1940).

For the full text of the foregoing proposal, see Bonnet, 1950, *Bull. zool. Nomencl.* **3** : 178-179.

DOCUMENT 1/5

By the late LODOVICO DI CAPORIACCO
(*University of Parma, Italy*)

Statement dated 19th January 1948

LAW OF PRESCRIPTION : COMMENTS ON PROFESSOR P. BONNET'S PROPOSITION No. 2

Proposal No. 2 of Professor Bonnet (proposal to add a new Article (Article 27 bis : Law of Prescription)) : " No name which has remained ignored for many years can, for reasons of priority, replace a generic or trivial name which has been utilized for a long time : if such a case arises, specialists will decide."

I agree with the intention of the proposal, but this seems to me formulated in too vague a manner : *many years* is subject to personal, i.e. variable, interpretation ; I think it is necessary to fix the time which gives the right to prescription : for instance, one might fix 30 years, or, say, 50 consecutive papers in which, instead of the oldest name, a later one has been used. It seems to me impossible to agree with the proposal to let " specialists " decide in dubious cases, because how can we decide *who* is a specialist ? Dubious cases must be decided by the Commission, who, of course, listens to the opinion of specialists.

DOCUMENT 1/6

By the late LODOVICO DI CAPORIACCO
(*University of Parma, Italy*)

Statement dated 19th January 1948

COMMENT ON PROFESSOR P. BONNET'S PROPOSITION No. 3 RELATING TO RELAXATION OF THE LAW OF HOMONYMY IN RELATION TO GENERIC NAMES

Proposal No. 3 of Professor Bonnet (proposal to modify Article 34) : " A generic name must be rejected as a homonym only if it has been utilized for another genus in the same major group of animals or in the same country," etc.

I am absolutely *against* this proposal, which would be perhaps convenient in purely systematic works, but would cause great trouble and confusion in faunistic or other papers. For instance, a genus may be discovered in a country where another genus of the same name does not exist ; then the name of the discovered genus, though identical with the name of another genus, would be valid ; but afterwards the " homonym " genus may be found to extend also to the country where species of the first-named genus occur ; then the second genus would have to change its name. That would only cause confusion. Moreover in the case cited by Professor Bonnet, the name *Argyrodes* (Araneae) ought to be changed in any case, even if the proposal of Professor Bonnet were to be accepted since *Argyrodes* (Lepidoptera) is found in Hungary, which is a part of Europe, where *Argyrodes* (Araneae) is found also.

DOCUMENT 1/7

Proposal submitted by Dr. HENNING LEMCHE on behalf of a large group of Scandinavian Zoologists

Law of Prescription

The proposal (received on 7th June 1948) for the incorporation into the *Règles* of a Law of Prescription submitted by HENNING LEMCHE on his own behalf and on that of a large group of Scandinavian zoologists for consideration by the International Commission on Zoological Nomenclature at the meeting to be held in Paris in July of that year during the Thirteenth International Congress of Zoology.

The provision so proposed to be adopted was the following :—

> A name of a genus or species, given before the year 1850 and not used in scientific literature since 1st January 1850, is to be rejected in favour of such other name which has been in general use for the genus or species in question since that date.

For the full text of the foregoing proposal, see Lemche, 1950, *Bull. zool. Nomencl.* **3** : 158-161.

DOCUMENT 1/8

Statement furnished by

SMITHSONIAN INSTITUTION COMMITTEE ON NOMENCLATURE

Letter from Dr. CURTIS W. SABROSKY dated 25th June 1948

On the use of the Plenary Powers to Establish Nomina Conservanda

I am transmitting herewith, on behalf of the Smithsonian Institution Committee on Nomenclature, a statement and petition " On the Use of the Plenary Powers to Establish Nomina Conservanda." Because of delay in publication in *Science* and the consequent impossibility of thus securing many reactions to the petition in time to transmit them to you before the Paris Congress, we distributed mimeographed copies in order to secure representative opinions on the matter. All original material—signatures, comments, and letters—received in response to this solicitation are enclosed herewith, together with a copy of the mimeographed circular and a reprint of the statement as it appeared in *Science*.

In order to assist you in interpreting the signatures, I have prepared a list of the persons, grouped by institution or organisation, with the general field of study indicated for each person as Zoology, Paleontology or Entomology.

In summation, 156 (Group I) approve the petition as it stands (or with minor comments or reservations that do not seriously modify their general approval), 7 (Groups II and III) would either go much farther than this petition in restricting and limiting Suspensions, or would eliminate Suspensions altogether, and only 5 (Group IV) oppose the petition and stand strongly in favour of Suspension. One of the latter, Dr. Forbes, perhaps does not favour Suspension, for his opposition is on a different basis. Two reactions are difficult to classify.

Annexe to letter submitted on behalf of the Smithsonian Institution Committee on Nomenclature

Reprinted from *Science*, 21st May 1948, Vol. 107, No. 2786, pages 543-544

A PROPOSED PETITION TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

The numerous applications now before the International Commission on Zoological Nomenclature, requesting exercise of the Plenary Powers for Suspension of the International Rules and establishment of a number of *nomina conservanda*, are commanding considerable interest among zoologists. The large number of applications, it is true, has resulted in part from the long hiatus in nomenclatural activity followed by a postwar revival of interest in nomenclature. Nevertheless, the number has become overwhelming, both for the Commission members who must study them and pass upon their merits and for zoologists who should give them thorough consideration in order that the Commission may have the benefit of various points of view. Some applications, moreover, are too briefly presented and inadequately justified, though the burden of proof should rest on the applicant.

It is generally recognised that letters from scattered workers are a less effective means of demonstrating approval or disapproval than expression by groups of workers. Accordingly, the undersigned subcommittee of the Smithsonian Institution Committee on Zoological Nomenclature, composed of taxonomists of the U.S. National Museum, the Bureau of Entomology and Plant Quarantine, the Geological Survey, and the Fish and Wildlife Service, has drawn up the following statement, in the hope that it will crystallise opinion and serve as the basis for representations to the Commission. The full Committee has approved the petition and directed the sub-committee to publish it for consideration by other zoologists.

Zoologists are invited and urged to send their reactions to the petition to the undersigned as soon as possible, with the understanding that the Committee will transmit this petition together with all names and comments, both supporting and disagreeing, to the International Commission. All communications at hand will be sent to the Commission in time for the International Congress of Zoology at Paris in July 1948. In order to aid the work of the Committee and to avoid misinterpretation, it is requested that, if possible, a brief and unambiguous statement of approval or disapproval be given, though as much other comment may be added as the individual wishes.

On the Use of the Plenary Powers to Establish Nomina Conservanda

It is not the immediate purpose of the undersigned to discuss the merit of individual proposals now pending, but to present their views on the general question of the use of the Plenary Powers by the Commission.

We believe that the International Rules of Zoological Nomenclature offer a sound foundation for ultimate stability and uniformity. In theory, at least, a standard to which there is no exception is a more solid basis than a standard whose application is weakened by numerous exceptions. In most cases, taxonomists could apply the Rules strictly, with little or no confusion and only a little inconvenience. On the other hand, we recognise that due consideration should be given to common usage of important names by the great body of nontaxonomists, the general zoologists, physiologists, medical men, teachers, and applied zoologists of all kinds. It is reasonable to conclude that the overall objectives of stability and uniformity would be distinctly furthered if zoologists would agree upon some basic standards satisfactory to both viewpoints.

In the case of a specific or generic name transferred to a different species or genus (e.g. the transfer of the specific name *malariae* from the quartan malaria parasite to the malignant tertian, if the Rules are strictly applied), both the element of inconvenience and that of confusion are involved. Such a situation is likely to present adequate grounds for suspension of the Rules. Misidentified genotypes commonly result in transfers, though not all such cases are of sufficient importance to merit suspension.

Changes resulting from straight synonymy usually cause inconvenience rather than confusion. Experience indicates that even apparently serious changes of this nature can soon become commonly accepted (e.g. the change

from *Stegomyia fasciata* to *Aëdes aegypti* for the yellow fever mosquito). Such changes, accordingly, are not grounds for Suspension unless better reason can be demonstrated.

If too many suspensions are allowed, or if suspensions are allowed on weak or unconvincing evidence, the value of the Rules as a basis for stable nomenclature would be depreciated. We are convinced that the International Commission should as soon as possible reassure zoologists that such will not happen. We therefore petition the Commission to set forth some general standards concerning applications for suspension of the Rules and respectfully suggest the following for consideration :—

(1) Applications for suspension of the Rules should be *required to demonstrate* that strict application of the Rules would result in *far-reaching and substantial confusion* in the taxonomic use of names and/or in a lamentable change that would *greatly confuse* the literature of a related field (e.g. ecology, malariology, stratigraphic paleontology).

(2) Demonstration of confusion should include adequate bibliographic data showing the amount of usage and the percentage of uniformity. Mere assertion of " greater confusion than uniformity," unsupported by these data, should be considered inadequate.

(3) The Secretary of the Commission should return for additional evidence any application that does not contain the above demonstration and data.

(4) When an application that appears to satisfy the above requirements has been received and notice of its pending consideration has been published, the Commission should on its own initiative actively solicit—not merely passively await—the advice of specialists or other interested and qualified persons or groups from a variety of countries, museums, societies, and educational institutions.

Concerning the method to be employed by the Commission in actively soliciting advice on cases under consideration, it is suggested that, in addition to the regular published invitation to all interested individuals, use be made of existing national, society, and institutional committees on nomenclature, or that special panels in the various branches of taxonomic zoology be established for the purpose. These panels might be headed by, or liaison with them maintained by, various members of the Commission, in order to relieve the Secretary of the extra burden of these contacts.

Curtis W. Sabrosky and R. I. Sailer.

U.S. National Museum, Washington, D.C.

Editorial Note

The foregoing application reached the Office of the International Commission on 12th July 1948, and was too late, therefore, to be considered by the Commission when it met in Paris nine days later. Among the proposals which had previously been received and which were discussed in Paris was a proposal

submitted by Dr. Henning Lemche on behalf of a large group of Scandinavian zoologists in favour of the introduction into the *Règles* of a Law of Prescription, which, if then adopted, would have limited the scope of the Law of Priority to a much greater extent than did the existing Plenary Powers of the Commission which it was the purpose of the application by the Smithsonian Institution Committee on Nomenclature to curb. It was because, for the reasons explained above, that application was not then before the Commision that during the discussions in Paris, I drew attention to that application, in my capacity as Acting President of the Commission and President of the Section on Nomenclature, and supported the proposal, which was then adopted, that the whole question of the means to be taken for promoting stability in zoological nomenclature should be deferred until the next (Copenhagen) Congress in 1953 and that in the meantime the whole subject should be subject to comprehensive study with interested specialists (1950, *Bull. zool. Nomencl.* **4** : 233).

2. The application discussed above was resubmitted to the International Commission in 1952 under cover of a letter dated 16th July, from Dr. Richard E. Blackwelder, who had succeeded to the position held in 1948 by Dr. Curtis W. Sabrosky, as Secretary to the body which had in the meantime been reconstituted under the title Nomenclature Discussion Group, Washington, D.C. The note covering this resubmission which was enclosed with Dr. Blackwelder's letter will be found as Document 1/42 in the present series (*see* pp. 73-75).

(intl'd) F. H. 25 February 1953.

DOCUMENT 1/9

Views submitted in July 1948 on behalf of the ZOOLOGISCHES INSTITUT DER UNIVERSITÄT WIEN, the NATURHISTORISCHES MUSEUMS WIEN, ZOOLOGISCHE ABTEILUNG, and the ZOOLIGISCH-BOTANISCHE GESELLSCHAFT

Editorial Note: Attention is drawn to a statement urging the need for securing greater stability in zoological nomenclature which was placed before the Thirteenth International Congress of Zoology at Paris in July 1948 jointly by (1) the Zoological Institute of the University of Vienna; (2) the Zoological Department of the Naturhistoriches Museum of Vienna and (3) by the Zoologisch-Botanisch Gesellschaft of Vienna. The text of this communication was published in 1950 (*Bull zool. Nomencl.* **5**: 77–78.)

It will be seen that the request put to the Paris Congress by the Austrian zoologists was that there should be accepted a principle under which a name which had been consistently applied to a given animal should not be changed in cases where the defect in that name was solely of a technical nomenclatorial character. The following is the text of the proposition which was then submitted by the Austrian zoologists :—

> Jeder heute einheitlich gebrauchte, eingelebte wissenschaftliche Tiername ist ein unschätzbarer nomenklatorischer Wert, ein Verständigungsmittel, dessen Zerstörung den Zoologiebetrieb schwer schädigt. Bis zur endgültigen Regelung der Verhältnisse ist daher jede Aenderung eines einheitlich gebrauchten Namens zu unterlassen, wenn für die Aenderung nur formal nomenklatorische (Prioritäts-)Gründe, aber keine systemtischen Notwendigkeiten vorliegen.

DOCUMENT 1/10

By L. T. S. NORRIS-ELYE

(*The Manitoba Museum, Winnipeg, Man., Canada*)

Enclosure to a letter dated 20th October 1948

TYPE LOCALITIES

The type localities are determined by the locality from which the first species or sub-species were named and described; among many of the old authors, this locality is not named or the locality given is virtually useless, e.g.," Hudson Bay region " or " Eastern North America."

When most of the types were established, even many of those determined today, the ranges were rarely known and may not be finally delimited for many years to come, due to the lack of wide-spread collecting followed by competent taxonomic work; this may not be so in those cases where the type specimen is represented by a large series that formerly went under another name and has been shown to be another sub-species.

Until the respective ranges can be determined accurately, it is impossible for the taxonomist to select, for comparative purposes, really typical material from the central regions of the ranges, where intergradation is likely to be absent or, at least, at its minimum.

Under our present system of selecting the typical form, our model is not necessarily the purest in form, but must be the specimen that was first described, perhaps barely within what turns out later to be its range; it may well be an intergrade in point of fact. It was determined by chance rather than knowledge; this system exists almost throughout the whole field of zoology.

Had the type of *Peromyscus maniculatus bairdii* (Hoy & Kennicott) been taken, say, at Pelican Lake, Manitoba, instead of at Bloomington, Illinois, most of the topotypes would have been quite unrecognisable by the pelts, on account of their intergradation with *Peromyscus maniculatus osgoodi* Mearns which does not become really typical for about one hundred miles to the west. At Pelican Lake, fifty specimens may well show over a dozen marked variations; such instances could be duplicated by the hundreds.

The difficulties of the taxonomist are often much increased by this situation and any device that will lessen them, without creating new problems, should be given some consideration. The rules give no protection against two types of sub-species being described within a very few miles of each other, perhaps both of them being intergrades.

With the full realization that long-standing rules in world-wide use cannot be discarded, without making the remedy worse than the disease, the writer ventures to offer a suggestion that might ensure that comparisons would be based upon a much greater factual knowledge by using only material that is the least tainted by intergradation.

It is proposed that the types be retained, largely for (1) recording data as to discovery, description, priority, etc., and (2) for comparative purposes, as at present, but only until enough range data shall have been secured to show practically the whole range, without any reasonable likelihood of the discovery of any serious extension of the ranges so far discovered.

When the ranges can be reasonably well delimited, the centres of those ranges could be determined mathematically ; from such centres, radii could be established to form areas to be called " centros " or some such name and all specimens taken within that area could be called " centrotypes " or some such name. Here, priorities of nomenclature would not interfere with the name stability, except when the type name had to be changed ; there would be no first-named centrotype.

With such an accessory system, consideration might well be given to the following possible advantages :—

(1) Centros could be established for some of the vague type localities.

(2) It would be impossible to establish centrotypes until really adequate work had been done on the entire ranges, thus ensuring eventual stability.

(3) The centrotypes must be more truly typical than many or most of the present types in all characters, pelage, dental and cranial.

(4) The system should put some curb on those who rush into print to name allegedly new sub-species, before becoming acquainted with some of the most fundamental facts ; these tendencies are even far worse in the case of naming new species where intergrading is not recognised.

(5) It should lessen the difficulties of the taxonomist and greatly increase the finality of his determinations.

(6) It would make the assessment of degrees of inter-gradation of border-line specimens much more accurate and more obvious.

It may be questioned what would happen if the " lumpers " or " splitters " got to work on some adjacent ranges ; the answer seems to be that a system of centros would meet the situation immediately by means of projecting new radii and forming a new or several new centros.

DOCUMENT 1/11

By THORD RAGNAR NORDENSKJOLD
(*Stockholm, Sweden*)

(1) Enclosure to a letter received on 25th May 1949

PROPOSALS FOR THE SETTLEMENT OF CERTAIN QUESTIONS OF NOMENCLATURE

A name of an insect which has become the accepted term among scientists should not automatically be supplanted or replaced by another name given by an earlier author but found later in scientific literature. The term originally used—" the most usual name " of the insect—could in many cases be retained as a synonym and constitute a *nomen quasiconservandum*, to be included and preserved in a (newly created) entomological central register. This *nomen quasiconservandum*, which is always to be regarded as a complement to the author's name and is given only secondary importance, should, when the insect is named, always be given in a footnote or within brackets after the older author's name which had been discovered later and which, in accordance with the principle of priority should be regarded as the original main name of the insect—as *nomen primum auctoris*. Alternatively, it is proposed that the Congress should recommend that the lists of synonyms at the end of every future scientific work should be so constructed that *nomina prima auctoris* are placed first and *nomina quasiconservanda* second (the latter followed by " (n.qc.) ") among the number of synonyms for every species of insect. Whether the traditional name can be considered to possess such strong goodwill that it is really worth while retaining it for the future and codifying it should be decided in the first instance by a body chosen by the Entomologists' Congress but functioning also between Congresses. The final decision should be made by the International Congress of Entomology.

The creation of a central entomological register is very desirable. In addition to the listing of *nomina quasiconservanda*, its task should be to compile complete lists of synonyms, including all names for every species of insect used in scientific literature. In these lists the *nomen primum auctoris* (the oldest known author's name for the insect concerned) should be printed in heavy type and the *nomen quasiconservandum* in spaced-out heavy type.

The adoption of *nomina quasiconservanda* could alternatively be of a provisional character and be retained in each individual case only until the rediscovered original author's name has gained sufficiently wide acceptance. One might also consider a 50-year provisional arrangement, to remain in force until a firm new principle has been established regarding the use of *nomina quasiconservanda*. It should be noted that, in certain cases, it may be easier to reach agreement about the adoption of two names (a *nomen auctoris* and a *nomen quasiconservandum*) than of one (a *nomen primum auctoris* only). For the sake of uniformity, it would be better to use two names simultaneously for an insect than for different authors to use different names for the same insect in scientific works.

In the hope that some entomologist will adopt my proposal and that it will be considered as the basis for further discussions and to some extent improved upon, I herewith submit it for consideration to the International Nomenclature Commission.

(2) Letter dated 20th December 1952

In May, 1949, I wrote to you about the stabilization of zoological nomenclature (in respect of the species). I was mainly concerned with the nomenclature of the insect groups and my intention was to make a contribution towards solving the " nomina conservanda " question.

In this letter, which I hope will be put together with my earlier one—already, as I understand, in the dossier for new suggestions for nomenclature—I wish to develop further my earlier and perhaps somewhat confused line of thought and to some extent amplify and adjust it (in order to avoid difficulties in the practical application of my suggestions).

Assuming that an insect has been named in a certain way for a century or so, but that a subsequent discovery has shown that an author some fifty years earlier for instance has satisfactorily described the insect in question and given it a different name, causing this last-mentioned *primum nomen*, in accordance with the law of priority, to be raised to the insect's *nomen auctoris*, then the name that has been in general use during the hundred years or more should, in accordance with my 1949 suggestion, be retained as a subordinate attribute (a *nomen quasiconservadum*) in conjunction with the author's name for at least fifty years after the discovery of the " new " author's name.

A binominal system with regard to the insect would thus be involved to some extent. To this the following objections may be raised :—

(1) This new system would be clumsy and unwieldy.

(2) An insect can never be designated by more than *one* name. A double naming would be an absurdity in itself.

(3) Owing to the accurate indications of synonyms provided and supervised by present-day specialists on different animal genera and owing to the existing practice of setting out, often in parentheses, during a certain transitional period, the earlier-used insect name after the " new " author's name, the proposed arrangement is superfluous.

(4) In certain cases it would be difficult to decide upon *one nomen quasiconservandum*. Here, strictly speaking, *two* or even *three nomina quasiconservanda* would have to be added as attributes to the *nomen primum auctoris*. Such an arrangement would obviously be preposterous.

In this connection the following may be said :—

The binominal system, which of course is not absolute but only conditional, can be adopted in the lists of synonyms given in the scientific works. This method would prevent unwieldiness in the text of scientific works where an

insect's name recurs frequently, would reduce editorial difficulties, would bring down costs and would avoid any great deviation from current usage in the production of scientific works.

In that case one thing would be essential: the *nomen primum auctoris*, as such, would have to be indicated in some way *typographically* and preferably also *acoustically* (when spoken). Here, then, it would be appropriate to print the Latin word of the insect name in *spaced-out* type and (or) to add to it the word (*prim.*), an abbreviation of (*priumum nomen auctoris*). This (*prim.*) would have the same effect as an "NB," as a "Warning." It would be an urgent request: "Look at the synonym list in this book."

In such a system the synonyms, as hitherto, would be arranged by priority, with one possible exception; viz. the *nomen* interimistically authorised as the *nomen quasiconservandum* by the International Nomenclature Commission or any of its special committees would always—even irrespective of its priority, by virtue only of its actuality—be placed immediately after (closest to) the oldest name of the insect and would by its fat type be specially characterised and distinguished among all the other synonyms as a *suprasynonym* (provisionally for only fifty years, let us say, if the entomologists so desired), after which the pure, *simple*, original nomenclature would be restored. The "new" *nomen primum auctoris* of the insect could then be regarded as having gained a sufficiently permanent footing to be able to stand alone by itself. The *nomen quasiconservandum* could be entirely dispensed with as being superfluous in the future.

The specialists of today perhaps may not find the above line of thought particularly obvious or desirable. Nevertheless, I venture to express this my opinion not because I am in a modest way a specialist on the Hemiptera and Heteroptera, but in the first place because I am a practical entomologist, a plant protection entomologist and in the second place a zoologist (general) and biologist. I present my proposal with the following motto: "A stabilized nomenclature for the species is a just and reasonable demand on the part of the practical entomologist or zoologist and biologist generally." With the present arrangement the latter cannot get a proper survey, even if the specialist can just manage to do so.

What would be the real innovation, the real change, if my proposal were adopted?

Clearly, those who wished could consistently employ the binomial system in all connections, written as well as spoken.

The use of the "double name" would, however, only be definitely imposed, if one can use such a term at all in the case of scientific congresses, in the following circumstances:—

(1) When using in speech the name of the insect belonging to the category in question.

(2) On the insect labels in scientific collections where there is no clear reference to the list of synonymns in a particular work (i.e. where the entire collection is not arranged according to some definite, clearly indicated, scientifically systematic work).

DOCUMENT 1/12

By W. D. LANG, D.Sc.

(formerly of *British Museum (Natural History), London*) (*Bridport, Dorset, England*)

Paper dated 13th June 1951

SOME COMMENTS ON THE CODE OF ZOOLOGICAL NOMENCLATURE APPROVED BY THE INTERNATIONAL CONGRESS.

(1) The object of the Code is to get uniformity in Zoological Nomenclature, and so to frame the rules that they can be applied in only one way, thus leaving no loophole for a subjective interpretation.

This I hold to be impossible of attainment. However much the Code is simplified, the whole subject (to judge from a life-time's experience) is bound to be so complicated and to leave so much to individual interpretation, that anything like complete agreement among zoologists (apart from those who are unwilling to submit to any rules imposed from without) must for ever remain, in my opinion, only an ideal and an aim practically unattainable.

(2) If this is conceded, the only alternative to the present state of affairs is to add dictatorial powers to the International Commission, that is, to convert their present function of standardising certain names on regulated appeal, to the duty of ultimately standardising all names. A zoologist could then simply apply to the Commission for the standardised name of a given species. I think that most zoologists would gladly accept such an innovation, which, during their lives, would save them months, if not years, of time ; and would grieve only those who regard the authorship of a name, not as a matter of general convenience, but as involving a moral right.

(3) Should such a change be made, I admit that the practical difficulties might be great, but should not be great enough to condemn the proposal without over-mastering reasons.

(*a*) First, it would greatly increase the work of Commission, and ways and means would have to be found of increasing the staff. Since the proposal benefits zoologists, it would be up to them to produce the means.

(*b*) In giving the Commission dictatorial powers, it is understood that in deciding what is the proper name for a genus or species, the Commission would be guided by the general principles inherent in the present Code.

(*c*) To decide upon the proper name of a species pre-supposes that the author who originally gave the name applied it to one species only ; or that where the original name covers more than one species, that either a type-specimen was originally designated, or, if not, that a lectotype has since been chosen, or, if the type-material has been lost, a neotype has been instituted. On the other hand, this might not have been done, and in that case the Commission itself should designate a lectotype or neotype. There would then be no uncertainty about the type specimen, and therefore about the species to which the standardised name is given. It would make the identity of a species a *matter of fact ;* for whatever is, or is not, a given species, the type-specimen,

by definition, is that species ; and whether or not any other specimen belongs to that species, must remain a *matter of opinion* varying with the different views of individual zoologists. The standardised name must be applied to the type-specimen, also standardised, if necessary.

(*d*) As with the species, so with the genus. The Commission should standardise all generic names with reference to the type-*species* (itself founded upon the type-*specimen*). More often than the specific type-specimen, has the type-species not been designated, either originally or subsequently, and in many cases has proved impossible of determination, except on debatable grounds. A dictatorial ruling on the generic name to be used and the type-species to be accepted would be a boon to zoologists, probably to a very large majority.

DOCUMENT 1/13

By CH. FERRIÈRE
(*Genève, Switzerland*)

Paper submitted through H. BOSCHMA (Leiden) under cover of a letter dated 9th August 1951

(This paper was presented to the Ninth International Congress of Entomology, Amsterdam, August 1951)

LE FARDEAU DES VIEILLES ESPÈCES INCONNUES

Dans plusieurs groupes d'insectes il existe un grand nombre d'espèces décrites par des auteurs anciens et qui sont restées depuis leur époque complètement inconnues. Leurs descriptions incomplètes ne permettent pas de les reconnaître et les types n'ont plus été étudiés. Des exemples nombreux de ces espèces qui encombrent les catalogues sont indiqués parmi les Hyménoptères Chalcidoidea, à l'occasion d'une étude monographique entreprise pour les espèces d'Europe. Des centaines d'espèces décrites par Fabricius, Spinola, Dalman, Fonscolombe, Walker, Westwood, Nees, Förster, Ratzeburg, et d'autres, sont impossible à reconnaître et sont le plus souvent placées dans des genres auxquels elles n'appartiennent pas. Que faire de ces espèces, pour la recherche et l'étude desquelles une vie humaine ne suffirait pas ?

Nous voudrions voir adopter par les systématiciens les propositions suivantes :

1. Toute espèce qui a été décrite il y a plus de cent ans, n'a jamais été retrouvée depuis lors et ne peut pas être reconnue d'après sa description, ne doit plus être prise en considération.

2. Si le type est perdu, cette espèce doit être rayée des catalogues et traités comme si elle n'existait pas.

3. Si le type existe encore, il ne pourra être rendu valide qu'après avoir été redécrit dans le genre auquel il appartient vraiment. Le nom du premier auteur est alors mis entre parenthese et le nom de l'auteur qui a fait la nouvelle description est indiqué a la suite.

4. Dans le cas de la redescription, par un auteur récent, d'une espèce restée inconnue et vieille de plus de cent ans, la question de la priorité en cas de synonymie est à discuter.

DOCUMENT 1/14

By the late K. W. DAMMERMAN
(*Rijksmuseum Van Natuurlijke Historie, Leiden, The Netherlands*)

Paper submitted through Professor H. BOSCHMA (Leiden) under cover of a letter dated 9th August 1951

(This paper was presented to the Ninth International Congress of Entomology, Amsterdam, August 1951)

PROPOSALS CONCERNING THE NOMENCLATURE OF FAMILY NAMES AND OF NAMES OF ECONOMICALLY IMPORTANT INSECTS

I. 1. To restrict the ever continuing changes of names and to arrive at a greater stability in nomenclature, it is advisable to obtain as soon as possible a fixation, in the first place of family names of insects and of the names of the most important insects of economic interest (noxious as well as beneficial species).

2. In this fixation the names that were in general use at the time of the beginning of the *Zoological Record* (1864), and that generally were in use till the introduction of the International Rules (1905), as far as possible are to be stabilised. Moreover, it is very advisable that attention should be paid to the *nomina conservanda* proposed by Apstein (1915. Nomina conservanda ; *Sitzb. Ges. naturf. Fr.* **5** : 119) and by Handlirsch (1915. In : *Schröders Hand. d. Entomologie* **3** : 79). (See also Dammerman, 1950, Continuity *versus* priority in Nomenclature, *Tijdschr. v. Entomologie* **92** : 34.)

II. 1. To arrive at results within reasonable time, the following procedure is recommended :

During three years, starting with 1952, or as many more as will appear necessary, an attempt shall be made to fix at least ten names of families belonging to each of the following groups of insects : Hymenoptera 10, Coleoptera 10, Lepidoptera 10, Diptera 10, Rhynchota 10, Orthoptera, Thysanoptera, Mallophaga and Pediculina together 10, other Orders of insects 10 ;

Moreover, at least ten names of economically important insects belonging to each of the seven above-mentioned groups, and for each of the six following geographical regions : Europe (or, as the case may be, the Palaearctic Region), Africa, the Oriental Region, Australia, North America, and South America.

2. In the case of family names a beginning shall be made with the names that were in common use about 1864.

III. 1. The present Congress is requested to appoint an international committee for stimulation and co-ordination of the work referred to in paragraph I above. This committee shall solicit co-operators or organisations to assist in this task. Individual investigators willing to co-operate shall be free to apply to the committee.

2. Co-operators shall observe the following rules :

a. If an economically important insect is at the same time the type species of a genus from which a well-known family name is derived, the reviser of this family name shall communicate directly or through the above-named committee with the person who is charged with the fixation of the name of the insect concerned, in order to arrive as soon as possible at a unanimous proposal.

b. The same procedure shall be followed in the case of an insect which is of great economic importance in more than one of the main geographical regions.

IV. Any entomologist or zoologist who detects a new name which on the basis of priority would involve a change of a well-known family name or of the name of an insect of economic importance, shall abstain from introducing that name, and shall immediately communicate with the committee referred to above. This committee shall send the proposal for further consideration to the co-operator concerned.

V. 1. Every year the names dealt with by the co-operators shall be published, if possible after having obtained concurrence of the majority of an existing national committee on nomenclature or of an existing national organisation of entomology.

2. The present Congress will be requested to obtain permission for publication of the proposals mentioned above in the English language in the *Bulletin of Zoological Nomenclature.*

Moreover by announcement in the above named *Bulletin* or in whatever journal the present proposal is first published all other journals suitable for the purpose shall be requested at least to publish the proposed names, with a reference to the number of the *Bulletin* or to the other publication concerned.

3. One year after publication of the proposals, when the opportunity for discussion may be deemed to have been sufficient, the above-mentioned committee shall take a decision concerning the proposed names. All names having obtained the concurrence of at least two-thirds of the members of the committee shall be published at once, and zoologists will be requested to use in future these names, whilst the International Commission on Zoological Nomenclature will be requested to place these names as soon as possible on the *Official List.*

4. Names that have not obtained a majority of two-thirds of the votes of the members of the committee, shall not immediately be rejected, but shall remain under suspension during one year. The committee shall then again consider these names. These names shall be rejected only when the committee can recommend alternative names to be used as the valid names in their place.

DOCUMENT 1/15

Proposal by RUDOLF RICHTER

(*Senckenbeigische Naturforschende Gesellschaft, Frankfurt-a-Main, Germany*)

The proposal dated 18th October 1951 submitted by RUDOLF RICHTER (Frankfurt a.M.) that all names placed upon the "Official List" should be validated under the Plenary Powers (1952, "Bull. zool. Nomencl." 6 : 184-185)

In the above paper Professor Richter advocated the adoption of measures to promote stability in zoological (and palaeontological) nomenclature and suggested that all names accepted in the forthcoming international *Treatise on Invertebrate Paleontology* should be placed on the *Official List*. The specific proposal submitted by Professor Richter was the following :—

> Alle Namen, die auf die Offizielle Liste gesetzt worden sind, sollen als endgültig geschützt gelten, unter Suspension der Regeln.

DOCUMENT 1/16

By D. SHENEFELT and J. T. MEDLER

(*Department of Entomology, University of Wisconsin, Madison, Wisconsin, U.S.A.*)

Paper submitted by the senior author under cover of a letter dated 9th November 1951

(This paper was published in *Science* on 10th August 1951)

SHOULD A "LAW OF RECENCY" BE ADDED TO THE INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE ?

Proposal, counterproposal, and debate on zoological nomenclature have been presented in numerous zoological periodicals during recent years. Basically analysed, the literature appears to represent a clash of opinion between two groups : those who feel that it is desirable to follow strictly Article 25 (the law of priority) and limit the commission's use of plenary powers, and those who advocate more extensive use of the plenary powers by the commission, with less strict application of Article 25.

Individuals in both groups admit that, at present, the procedures used are confusing and unwieldy. Each side concedes that there is some merit in the other's case. It is not the intention of the writers to enter into this debate ; opinions of the two schools are perhaps irreconcilable in this generation.

The writers do feel, however, that constructive thought should be given by all zoologists to ways of preventing similar confusion in the future, especially by eliminating the words, too frequently found in the *Opinions* and the proposals in the *Bulletin of Zoological Nomenclature*, that such and such action "would clearly result in more confusion than uniformity." The debates being waged in all zoological literature about the status of names clearly indicate that confusion is not entirely a matter of the future, but a real concern of the present.

An examination of the sources from which this confusion has arisen reveals that the important ones are :

(1) Lack of knowledge of the existence of a publication ;

(2) Lack of knowledge of the exact date of publication ;

(3) The " dragging-out " of questions by different schools of thought arising from differences in interpretation of the Code and its applications ;

(4) Deliberate refusal to give up a name once it has been learned—i.e.. deliberate failure to apply the law of priority and maintaining that its application will create " confusion."

Perhaps errors made in the past can be corrected only by rulings such as those being made by the commission under its plenary powers. But what about the future ? Is the commission to continue to use its plenary powers indefinitely (or be forced to do so by allowing the same causes of confusion to continue to exist) ? Is the commission to rule on its rulings as it apparently must do with the Meigen 1800 paper ?[1]

It is evident that something is wrong when the commission has to use its plenary powers so often. There is a need for practical working procedure in nomenclature to avoid multiple use of names, especially at generic and suprageneric levels. The writers believe that if stability is ever to be obtained in the use of scientific names a " law of recency " must be added to complement the law of priority. Once a name is rejected it should not be used again as a valid name unless there are definite reasons for so doing and they are given in publication. The proposed " law of recency " might be stated as follows :

The names used in the most recently published article dealing with the taxonomy or nomenclature of a group or species shall be regarded as authoritative *until further publication is made* reorganising or otherwise disposing of the names involved and *giving the reasons why the changes are made.*

The need for such a law can be amply demonstrated. For instance, in 1915 Gahan[2] definitely synonymized many generic names with *Opius* Wesmael. In 1943 Hincks[3] wrote :—

> *Bracon carbonarius* Nees, 1834, is designated as the genotype of *Opius* by Westwood (1840). This species is also the type of the monobasic genus *Biosteres* Foerster, 1862. For the present it will be better to transfer the whole of the species now grouped under *Biosteres* to the genus *Opius* until such time as an examination of the species leads to a regrouping. There are rather a large number of generic names available in this section, but it is evident that Gahan (1915) regards the divisions as of little generic value.

While Mr. Hincks' action appears to be correct, it illustrates the necessity of the proposed " law of recency," which would *compel* the transfer of the names in *Biosteres* to *Opius* and the acceptance of the synonymy given by Gahan until such time as the group was revised and *the reasons published* for changing the system used by Gahan.

The confusion that has existed with regard to *Bracon* Fabricius, *Cremnops* Foerster, and *Microbracon* Ashmead also illustrates the case in point. In 1917 Gahan[4] transferred the name *Bracon* to replace *Cremnops*. This name change was accepted in North America but rejected in Europe. Finally, in 1935, the

question was decided by the International Commission, but the results were not published until 1943.

Although illustrations have been drawn from the family Braconidae, numerous other examples could be given—e.g., *Agrion* versus *Calopteryx*.[5] Undoubtedly any taxonomist can readily cite similar cases in his particular field of interest.

At the present time, refusal to accept published synonymy is responsible for differences in accepted nomenclature between different groups or individuals. Such refusal and the resulting difference in accepted names mean that the cataloguer must constantly change species from one generic name to another and back as new articles appear—or carry the synonymy where he wishes, using his own interpretation in order to place the species in the " proper " place in his catalogue. The other alternative is to carry the species under several genera at the same time. In either case the situation produces much confusion and additional labour rather than stability.

Where does this leave the cataloguer ? In the first illustration mentioned, is Gahan's synonymy to be accepted ? Or are we free to ignore all but the original descriptions ? Should the individual have the right to be " conservative," reject the published work of others (perhaps by merely ignoring it or dismissing it by stating that it is incorrect) and continue to use names that have been synonymized by others ? For example, is Whiting's continued use of *Habrobracon juglandis* (Ashm.) valid ?

The writers believe that the adoption of such a " law of recency " would aid in lessening the confusion created by sources (3) and (4), since points of disagreement would be brought to the fore where they could be acted upon by the International Commission when necessary, resulting in a much more rapid stabilization of names than occurs under the present system.

Incidentally, the lone expression " in the opinion of the writer " does not convey the extent of similarity or difference existing between organisms and perhaps should not be regarded as a valid reason for changing the established status of a name. The same objections may be raised against " new synonymy " or " new combination " when used without descriptions of similarities or differences.

To eliminate sources (1) and (2) it is suggested that names and their changes be considered as fully validated only after appearing in the *Zoological Record*, that for validation purposes the author be required to send a copy or facsimile reproduction of his publication to this source, giving the date on which the article in question was distributed (i.e., " published "), and that this date be included after the reference in the *Zoological Record* under the " Titles."

Such a procedure would eliminate the question of the date of publication serving as a source of future trouble. That the publication date is still a source of confusion is illustrated by the fact that Sabrosky[6] found it necessary to publish a note regarding the date of " publication " of an article distributed as late as 1948.

It also appears desirable to provide for continuing international support (perhaps through Unesco or some similar means) for the *Zoological Record*

and to ensure sufficient funds so that an adequate staff may be maintained to produce a new volume within the year following that with which the volume deals.

It would be the authors' suggestion that the plenary powers of the International Commission be terminated at the point where the names and dates are first validated in the *Zoological Record*, i.e., that the plenary powers should not extend to names to be published in the future and that a date be set at which the application of the law of priority becomes rigid.

Since it is only through stabilization of the International Code and the procedures to be followed that nomenclature can be placed on a sound basis, the suggestions given are presented as a constructive criticism rather than from any derogatory motives. The authors would welcome both private communications and published discussions of the ideas herein submitted.

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DOCUMENT 1/17

By RICHARD MEINERTZHAGEN, D.S.O.
(*London*)

Need for Power to fix Type Localities

Extract from an application to the International Commission on Zoological Nomenclature received on 21st November 1951, relating to the type locality to be accepted for the Ostrich (Meinertzhagen, 1952, *Bull. zool. Nomencl.* **9**: 97).

> 4. It is a serious weakness in the *Règles* that they contain no provision for regulating the selection of type localities thus for preventing confusion of the kind described above from arising. It is very much to be hoped that this omission will be repaired by the Copenhagen Congress in 1953, when it resumes the discussions begun in Paris in 1948 for securing greater stability in zoological nomenclature. In the meantime effective action can be taken only by the International Commission on Zoological Nomenclature and it is for this reason that I am submitting the present application.

DOCUMENT 1/18

Document communicated by HOBART M. SMITH
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Enclosure to a letter dated 29th January 1952

Editorial Note : The paper communicated by Professor Hobart M. Smith which appears below is a paper written by himself jointly with Dr. Richard E. Blackwelder and Dr. J. Brookes Knight, which was published in *Science* **111** (No. 2881) : 289-290 on 17th March 1950.

Categories of Availability and Validity of Zoological Names

Recent publication by one of us (Smith, H. M., *Science*, 1947, **106,** 11) of a note on the use of the expressions *valid* and *available* in describing the status of scientific names prompted the other two to write him that experience in other groups of animals might modify the conclusions that he had reached. Comparison of usage in our three widely separated fields (herpetology, entomology, and paleontology) has led to substantial agreement on a set of terms and definitions different from those previously held by any of us. It is thought that these conclusions may be of interest to others, for the categories involved are not clearly understood by all taxonomists, and the terminology is often confused in practice. Particularly confusing are the uses of *valid* or *validly* by different writers for several of the categories.

Zoological names appear to fall into four categories in respect to their nomenclatural status. (1) All names that have appeared in print (in the broadest sense) must be considered for possible acceptance into scientific nomenclature. (2) Printed names that meet all the publication requirements of the *International Rules of Zoological Nomenclature* are automatically accepted into nomenclature. (3) Names published in full accord with the *Rules* are nomenclaturally acceptable if they are not pre-occupied by another name of the same spelling. (4) From among the nomenclaturally acceptable names, there is only one which, because it is the oldest or has been judicially accepted, can be properly used to the exclusion of all others under a given set of circumstances.

The first of these categories generally has not been given a name, although *printed*, *published*, and *occupied* have all been used. We believe that *printed* is not sufficiently descriptive since a printed label should be excluded, and *occupied* implies " in nomenclature " and so is more appropriate in the second category. *Published* appears to be logically applicable to all names that have appeared in print (in the broadest sense). Most published names are accepted into nomenclature, but some fail to meet requirements of the *Rules* and are disregarded in nomenclature ; examples are vernacular names, names without referrants (*nomina nuda*), and names printed in mediums not qualifying as scientific publications.

Names in the second category have generally been cited as *published*, but *available* has also been used. In the customary sense, however, not all these names are available for use, since some are junior homonyms ; and to be exact in this usage, *published* must be modified by " under the *Rules*." The term *occupied* may be applied appropriately to those published names that do

meet the requirements of the *Rules* as to publication. *Occupied* names include all named published according to the technical requirements of the *Rules*—all names that are accepted into zoological nomenclature, such as valid names, synonyms, homonyms, and *nomina inquirendae.*

The word *occupied* in this sense has an unfamiliar ring and may at first glance seem inappropriate. However, if one imagines that there is theoretically a niche for each possible combination of letters that could form a name under the *Rules*, and that when a name is published it would occupy its particular niche, the concept of occupation becomes clearer. Note also that the common use of the familiar term *preoccupied* makes it easier to understand the corresponding term *occupied.*

In the third category names have been described as *valid* or *available.* They are available for use, but they are not valid in the more common sense of that word, as being the one acceptable name. The numerous ways of using *valid* make it unsuitable for a sharply defined concept. We may then apply the term *available* to all names that were published in accordance with the requirements of the *Rules* (legally published) and which have not been so published previously for some other genus of animal or for some other species of the same genus. If any of them has been so published previously, the later name is said to be preoccupied and is called a homonym, or more significantly, a junior homonym, and is not available. Thus all names are available which are now properly in use or which may at any future time be properly used.

Finally the one name under which the species or genus is to be known has also been called the *valid* or *available* name. *Available* seems more appropriate in the third category, and *valid* has the disadvantage of being commonly used with several meanings. We suggest that the simple and self-expressive term *correct* be applied in a technical sense to the oldest available name for a genus or for a species within a genus. If the oldest available name has been set aside by the International Commission using its plenary powers, then the next oldest name or the one designated by the commission is the correct name.

The categories may then be defined and named as follows, according to these conclusions: *Any name that is printed and circulated is* published; *any published name that meets the publication requirements of the* Rules *is occupied* in zoological nomenclature (if it fails to meet the requirements it is an outlaw name, i.e., unpublished, illegally published, or a *nomen nudum*); *any occupied name that is not preoccupied by an older name of the same spelling is available* (if it is preoccupied it is a junior homonym and is not available); *the oldest available name is the* correct *name*, unless it has been specifically set aside by the Commission under the plenary powers. (The correct name will, of course, vary with changes in our knowledge of subjective synonymy or discovery of unknown facts in the history of the names, such as homonymy and objective synonymy. An available name whose genus cannot be identified is a *nomen dubium.*)

RICHARD E. BLACKWELDER,
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DOCUMENT 1/19

By PER BRINCK

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Letter dated 18th February 1952

On Type Localities and Allied Problems

Since a citation of a locality or a host does not constitute an indication, I cannot see that a correction of a wrong citation is a nomenclatorial matter *per se*. But of course it would be desirable to have a recommendation (or other arrangement) included in the *Règles* and as to the type localities it would be preferable to have the restriction effected by means of an Article similar to Article 30.

If a type is preserved a restriction (or correction) must be performed in accordance with what is known about the occurrence or life history of the nominal species as substantiated by the type. If the species is split up into subspecies, the typical race must be restricted according to the type. Well, this is quite clear. But what to do when there is no type left, that's the problem. Let us assume that a Spanish scientist makes a revision of a widespread European butterfly, named by Linnaeus. He found it worth while to distinguish a lot of races, named as subspecies by him. No Linnaean type is left and the locality is given as " Habitat in Europa." So far as I can see he can select as the typical race what he wants—even an isolated population in the Central Spanish mountains—in spite of the fact that it is quite impossible that Linnaeus had specimens from there when describing his species. Therefore, it would be desirable to have the restriction made so that it agrees with what the original author might have meant when describing his species. But this is not always so easy to fix and I suppose some trouble would arise from such restrictions. In the case of parasitic species a restriction (or a correction) ought to be performed in the same way, i.e. the revising scientist should choose a type host (etc.) which could have been given by the original author. A Mallophagan described by Nietzsche from *Corvus corone* but later found to be a typical parasite of herons, should have the type host corrected to a common European heron (as common and widespread as possible) and not to (for inst.) the South African Goliath Heron. If no host species was given in the original description, a similar recommendation to select the most common and widespread species in the country from which the author had his material (if known or presumed with some certainty) would be useful. For Denny, England. For Nietzsche, Germany. For Linnaeus, Sweden, etc.

DOCUMENT 1/20

By E. BERIO

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Enclosure to a letter dated 3rd March 1952

THE RULE OF PRIORITY IN ZOOLOGICAL NOMENCLATURE

According to Mr. Dammerman, who has criticized the so-called Law of Priority in *Tijdskr. Ent.* 92 : 34 (1949), after 50 years the hopes of reaching stability through the promulgation of the International Rules for Zoological Nomenclature have been disappointed, to the great distress of non-systematists who never know the proper name to apply to a given animal and the reason for this failure lies primarily in the retroactivity of the Law of Priority which, though introduced in 1905, operates retrospectively back to 1758, although authors prior to 1905 applied it only with reference to the most well-known works.

In the light of this view, it would be necessary to abolish such retroactivity, by starting the validity of the aforesaid Law of Priority from 1905 and by adopting, for the period prior to that year, the principle of continuity and prescription proposed by other people too.

Accepting Dammerman's proposal would lead to the adoption of the following principles :

(1) the Law of Priority must apply only to names subsequent to 1905

(2) names in general use between 1864 and 1905 are not to be replaced.

These are :

(*a*) firstly, names used in monographs or catalogues published between 1864 and 1905, which should be fixed by international agreement ;

(*b*) secondly, names in general use during the period above, this condition to be ascertained by an international commission.

(3) in doubtful cases, the Commission's opinion to be sought.

A list of catalogues and monographs (together with further details and instances) is given by Dammerman, whose premises, conclusions and proposals require careful examination.

I

Instability of nomenclature is not too serious an evil and its effects must not be exaggerated. Seeing an animal called by several different names may be vexatious and involve researches : no damage arises, however, so long as correct identification is possible.

One must recognise that even a not officially correct name fulfills its task perfectly, so long as there is no doubt what is meant ; non-observance of the International Rules calls for sanctions, but cannot prevent a name from designating an entity when its content is clear.

Thus, non-systematists need never trouble themselves about changes in designation, for they can use what names they like for designating the entities

they are dealing with, provided the name is clear, even if not orthodox or unobjectionable in relation to the International Rules for Nomenclature.

But systematists also are often perplexed at finding a well-known entity called by a name never seen previously, whether a re-exhumed name or a newly created one. In practice, when an author is compelled to change the status of a well-known denomination, he generally gives the synonymy for a certain time. Thus systematists keep up-to-date and non-systematists, after all, have but to do the same.

As will be seen later, there is one cause of nomenclatorial instability which does not depend upon strictly nomenclatorial conventions, but is due to systematic exigencies. Not even non-systematists can avoid these needs, although they often show a tendency to consider systematics as a quite subsidiary and negligible science and confound its field with that of true nomenclature which really is, on the contrary, a " handmaid of taxonomy."

II

But is the retroactivity of the Law of Priority really the main reason for continuous changes in denominations and names (1) ?

Generally speaking, changes of designation (2) take place in the following cases :—

I. Ascertainment of a change in the validity of a name

A name thought valid (3) may come to be thought invalid, and *vice versa*, owing to :—

(A) Information about the bionomiality of the work in which it was published ;
(B) Information about its synonymy ;
(C) Information about the date of its publication prior to or subsequent to 1758.

II. Ascertainment of a different status of employment (4)

(A) Ascertainment of the date, within the limits of the period subsequent to 1758, of the book in which a given name was published.
(B) Ascertainment of a content different from that commonly understood (5).

III. Rejection of a name

(A) Ascertainment of primary homonymy (6).
  (*a*) On ascertaining that two generic names are identical, the more recent one must be discarded.
  (*b*) On ascertaining that two original specific denominations are identical in their components, the more recent specific name must be discarded.

(B) Ascertainment of primary synonymy (7).
  (*a*) On ascertaining that two generic denominations have the same type-species (8), one must be discarded. This happens when, by using the Code's Rules to fix the type-species, more than one reviser is found who has chosen a given species as the type-species for different genera, or because the species fixed

as type is found not to be the one which the quoted references imply.

(*b*) On ascertaining that two specific names have been erected for the same entity, one must be discarded (9).

(C) Formation of secondary homonymy by transfer.

When a species is transferred to a genus including another species which possesses an identical specific name, two identical names occur, and one of the two names must be discarded as long as the two species are regarded as congeneric.

(D) Formation of secondary synonymy by transfer.

(*a*) When two species, each being the type-species of a generic denomination, are thought to be congeneric, one of the two generic names must be discarded, so long as the two species are regarded as congeneric.

(*b*) When two forms possessing different names are thought to be conspecific or identical, one of the names must be discarded for use as a component of the specific denomination (10).

IV. Rehabilitation of names

(A) Rehabilitation of a reputed primary synonym.

When it is ascertained that two genera possessing different names which had previously been regarded as primary synonyms, possess different type-species (see causes at item III, B, *a*), the later-published of these names is exhumed.

(B) Rehabilitation of a reputed secondary homonym.

When a species possessing a name which had previously been discarded on the ground of secondary homonymy, is removed from the genus, in which the secondary homonymy had occurred, the name which had been discarded is exhumed.* A similar situation arises when a species possessing a name, which led to the discarding, as a secondary homonym, of the name of another species in the genus is itself removed to another genus.

(C) Rehabilitation of a reputed secondary synonym.

(*a*) When a species which is the type species of a genus which has been rejected as a synonym of the name of another genus is removed to another genus, the name of the genus of which that species is the type-species is exhumed.

(*b*) When it is ascertained that two entities previously regarded as belonging to a single species belong to different species, the later-published of the two names—which had previously been discarded—is exhumed.

(D) Rehabilitation by reference to priority.

When a case of secondary homonymy arises, the oldest valid

Note by Editor:* Under a decision taken in Paris by the XIIIth International Congress of Zoology, a trivial name once rejected on grounds of secondary homonymy and replaced can never again be used for the same species. (See 1950, *Bull. zool. Nomencl.* **4 : 121–124.)

synonym of the name rejected on account of secondary homonymy is exhumed.

V. Creation of names

(A) Renaming. In the absence of a valid synonym, it is necessary, on the case of secondary homonymy arising, to erect a new name in replacement of the name then discarded.

(B) Separation of two entities previously confused with one another. When an entity formerly regarded as conspecific with another entity is found to be distinct, a new name must be erected for that species, provided that there is no valid existing specific name available for application to it. A similar situation arises when two genera formerly regarded as identical are found to be distinct, i.e., when the respective type-species of those genera are found not to be congeneric with one another.

VI. Composition of the denomination of a species

When a species is transferred to a genus in which it has never previously been placed, a new denomination for that species comes into existence, through the combination of the specific name of that species with the name of the genus to which it is transferred.

On examining the causes specified above, we at once observe that Nos. *I* (A, B, C), *II* (A, B), *III* (A, B) and *IV* represent changes of names which are due solely to nomenclatorial reasons, while in all the remaining cases the changes specified arise from necessities of a systematic nature.

We thus see that nomenclatorial instability may arise from two quite different causes, the one destined in time to cease, the other unavoidable, quite apart from the Law of Priority.

A first cause of instability is that specialists are still in the phase of research as to the priority of names, and they will continue to be so for a long time to come.

There are, for example, publications the date of which has not been established as yet, diagnosis or figures which have not been identified exactly, and works which have not been considered carefully, owing to their extreme rarity.

But these reasons of uncertainty are destined to cease sooner or later. When all the old names have been placed correctly both as regards date of publication and content, there will no longer be strictly nomenclatorial or formal causes for changes. Duplicate names will be known and such obstacles at last be over.

The highest stability will be reached in this direction by discarding definitively primary homonyms and synonyms, by ascertaining in the same way the validity, employment and content of names (by fixing without further uncertainties the type-species for generic names, and the type-specimen for names of species).

Against all the arguments about the time necessary to reach such stability must be set the knowledge that enforcing the Law of Priority in its official

manner must lead inexorably to such stability for, after all, the unique valid oldest name exists, and it only remains to find it.

A second and more important cause of instability in denominations and names arises from the connection of nomenclature with systematics.

Among the main principles of nomenclature there is one (see item *VI* above), arising from Linnaeus, which constitutes his particular innovation and colours the whole modern nomenclatorial conception, that is, that the binominal name applied to an entity must reveal the hierarchic systematic conception of the Author at that moment.

From this standpoint, nomenclatorial instability, as exemplified in cases *III* (C, D) ; *IV* (B, C, D) ; *V* and *VI*, is linked unavoidably to the flexibility of the system, being subject to continuous improvement and complication, and to differences of Authors' and Specialists' points of view.

The Law of Priority only affects such cause of instability as a last and logical consequence, for the changes of systematic position involve a change of denominations which, in its turn, leads to changes due to adaptation.

But this instability is fatal and incurable, for it is inevitable that both the system and systematics should evolve continuously and that no nomenclatorial rule can prevent instability from these causes, *so long as the rule of formal interdependence between names remains in force.*

III

The method of continuity and prescription which some people advocated even before the adoption of the present International Code and which Mr. Dammerman has now put forward with definite suggestions for its application could release nomenclature neither from the instability arising from the connection between nomenclature and systematics nor from instability arising from laborious research into the availability and use of names. Indeed, if such a method were now to be adopted after fifty years of the enforcement of the Rule of Priority, its adoption would undoubtedly add to the existing confusion.

Furthermore, as we could not in any circumstances cease to take notice of primary and secondary homonymy, the discovery of homonymies between the names used in catalogues, monographs and those used in other works would involve research of the same kind as that required under the Rule of Priority. But the task would be more complicated, for, in addition, there would be the difficulty of establishing whether authors who—acting logically —did not enforce a law of continuity when citing particular names, had intended to refer to the same entities as those to which those names were applied in the catalogue or monograph which had been adopted as the standard for the purpose of determining continuity or whether that they had applied those names to other species, considering that previously they had been misapplied by reason of erroneous identifications of the entities concerned.

In particular, the method of prescription could do nothing to prevent instability arising from changes in the system and in systematics of the kind referred to at the end of the preceding section. For, like the Rule of Priority, the principle of continuity and prescription leads exclusively to the selection of names which are used only as single elements in the compound denominations

used as the systematic names of species. The importance of this consideration becomes evident when it is recalled that systematic categories are subject to constant change and even at any one moment vary from one author to another. On this subject, however, it might be possible (as will be seen later) to obtain nomenclatorial stability, notwithstanding the instability of the system, if it were to be decided to keep the two elements separate from one another.

Turning back to the group of modifications in nomenclature which are independent of systematics, it will be noted that, among the many cases cited in Section II of the present paper, differences in the interpretation of the content of a name represent by far the commonest occurrence. That this should be so brings out clearly that the application of the principle of continuity would lead to exactly the same uncertainties as those arising under the Rule of Priority. The identification of primary homonyms and synonyms is relatively easy, either by research into the literature or by reference to such registers as Sherborn's *Index Animalium*, which are of great value for this purpose. But the chief cause of discordancy in the use of names arises from differences in their interpretation, for a novel interpretation may lead to a change of synonymies—often extensive in character, owing to the interdependence between entities and names. That this is so is well known to systematists, for everybody is aware that such inter-linking is the commonest reason for the changing of names.

In cases of this kind what assistance would be provided by the adoption of the method of continuity ? Evidently none, because either the interpretation of the catalogues and monographs accepted as the reference standard would involve uncertainties similar to those involved in the interpretation of the old names, or their interpretation would depend upon the sources cited in these works, in which case the situation would be the same as it is today.

Catalogues and monographs generally give, for purposes of identification, both bibliographical references and synonymies. This, for example, is done in the Staudinger-Rebel Catalogue of 1901 which Dammerman advocates should be taken as the starting point in the case of the Lepidoptera. But catalogues do not indicate under each specific name the fixation made for the type specimen of the species in question, and it would still be necessary, therefore, to go back to the diagnoses given by the original authors or to the type specimens, just as it is at present. Thus, no change in this matter would result from the adoption of the principle of continuity and prescription.

IV

The selection of monographs and catalogues as standards of reference for the purpose of applying the principle of continuity would thus not only involve injustices but would also give rise to uncertainties. In addition, difficulties would arise in ascertaining the so-called " general use " of a name during a certain period of time.

Finality and stability in such a matter could be reached only by rejecting all indeterminate concepts. The expression " general use " leaves room for a number of different shades of opinion and is thus open to objection. If applied in the most rigorous sense, this expression would require that within the specified period no author should have used another name to denominate

a given entity. The research involved in determining whether this condition was satisfied would give rise to a conflict between various possible interpretations of the content of a given denomination according to differences in the views held by individual authors, though, in practice, difficulties of this type might prove to be relatively infrequent.

The concept of duration in relation to the principle of prescription would also give rise to disputes in regard to continuity or discontinuity of usage in particular cases. This would be especially liable to happen in the case of names of species in those groups which, being less known, arouse no interest over long periods and possess only a scanty literature.

Finally, it would be impossible ever to lay on one side those works which are still models of perfection and a source of wonder. It would not be practicable to fix for all time an equivalence between the " official names " and the oldest names for given entities by means of a sort of synonymy between the two, for causes of uncertainty could not be abolished in this way.

To sum up, it appears that uncertainties in nomenclature arising from research into the question of the names which are employable under the Rule of Priority back to 1758—a class of investigation still in process of development —would be greatly increased by substituting some other rule in its place, especially a rule which would abolish the so-called retroactivity to 1758 which during the last fifty years has provided the basis for a definitive and concrete fixation of employable names. A change of direction at the present time would subvert recent monographs which represent the nearest approach to stability that is attainable in our present state of knowledge.

The present criticism of Mr. Dammerman's proposals gives me an opportunity, in my turn, to advance a proposition which has long kept me busy on the problem of zoological nomenclature and which, if adopted, would tend to remove one of the many elements of uncertainty in the zoological nomenclature of today. As will be seen later, this proposition provides some fixity for the names of species as against the constant changes necessitated by the requirements of systematics. It has, moreover, the merit of being, in practice, quite a simple remedy. As I have already pointed out, a complete freeing of nomenclature from systematics would provide the ideal means for stabilising nomenclature, for in the main names are changed for reasons of a systematic nature.

After ascertaining that, to take a particular example, the first name given to a particular species was " *Noctua chrysitis*," that name having been so given by Linnaeus, there could be no uncertainty if everyone were to cite that species only under the name " *Noctua chrysitis*," and, if subsequent changes in the systematic position assigned to that species were to be indicated without abolishing the original binominal denomination of " *Noctua chrysitis*." In other words, the binominal name " *Noctua chrysitis* " (which merely means the species *chrysitis* described by its original author—Linnaeus—as belonging to the genus *Noctua*) would be given a mere binominal of identification, if views were to change regarding the systematic position of the species so named. Thus, if, for example, the foregoing species were, on systematic grounds, to be transferred to (say) the genus *Plusia*, all that would be needed would be a

means for indicating briefly that " the species originally named *Noctua chrysitis* by Linnaeus belongs to the genus *Plusia*." What is required is a unique formula, as short as possible. For this purpose, the two formulae (the nomenclatorial and the systematic) would need to be brought together. This could be done in any of the following ways :—

1. *Plusia chrysitis Noctua* Linn.
2. *Plusia chrysitis* (*Noctua*) Linn.
3. *Plusia chrysitis* (*Noctua* Linn.)
4. *Plusia* (*chrysitis Noctua*) Linn.
5. *Plusia* (*chrysitis Noctua* Linn.)
6. *Plusia chrysitis* Linn. *Noctua*
7. *Plusia chrysitis* Linn. (*Noctua*)
8. *Plusia chrysitis* (Linn.) *Noctua*
9. *Plusia chrysitis* (Linn. *Noctua*)
10. *Plusia* (*chrysitis* Linn.) *Noctua*
11. *Plusia* (*chrysitis* Linn. *Noctua*)
12. *Plusia Noctua chrysitis* Linn.
13. *Plusia Noctua chrysitis* (Linn.)
14. *Plusia* (*Noctua*) *chrysitis* Linn.
15. *Plusia Noctua* (*chrysitis* Linn.)
16. *Plusia* (*Noctua chrysitis*) Linn.
17. *Plusia* (*Noctua chrysitis* Linn.)

The relative merits of the alternative positions 1-5, 6-11, and 12-17 and those of bracketing one or more terms depend upon the extent to which these methods bring into prominence the distinction between the systematic formula and the nomenclatorial formula. When an author's name applies both to the specific name and to the generic name, it may be useful for it to be placed after those names. Moreover, under the Code, where two generic names are cited in a given specific name, the second such name indicates the subgenus to which the species is referred ; accordingly, it may be appropriate for the original generic name to follow the specific name, rather than the systematic generic name. Since the most important element in the formula is the indication so given of the systematic position of the species and since there can be no doubt that the first term must be that of the systematic genus, it is evident that the best order to be given to the various terms is one or other of those comprised in the group 1-5.

It seems necessary to adopt bracketing to separate the two parts of the proposed formula. Moreover, the Code states that the name of the author of a specific name should be placed in brackets, when that species is transferred from the genus in which it was originally described to some other genus. Accordingly, a formula should be chosen in which the author's name is placed in brackets.

There remains therefore a choice between Formula 3 and Formula 5. Of these, there can be no doubt that Formula 3 is the better, for it brings into evidence the systematic formula at the most important point and in the most unitary manner, while at the same time making possible the addition of other indications recommended by the Code.

If Formula 3 were to be adopted, the position as regards (say) the species

described by Hübner in 1805 as *Noctua puta* and later transferred to the genus *Agrotis* Ochsenheimer, 1816, would be cited as :—

Agrotis puta (*Noctua* Hübner, 1805) Ochsenheimer, 1816.

By this notation, which is quite simple, nomenclature would be redeemed from systematics to some extent, for it would be possible to vary the generic name to any extent and as often as might be required on systematic grounds, but the second part of the formula would not be subject to change and would thus provide for securing the identification of the entity cited. The following are additional examples of the way in which the formula would operate. There are cases where the extended form shown in the second of these examples is really necessary.

(1) *Euxoa puta* (*Noctua* Hübner, 1805) Warren, 1909
(2) *Euxoa* (*Agrotis*) *puta* (*Noctua* Hübner, 1805) Dufrane, 1932.

This method of notation would be particularly useful where the same name is given to a number of non-congeneric species in the same work, as was often done, for example, by Bethune-Baker who published the following names in this way : *Lasiosceles pratti : Epa pratti : Zethes pratti : Targalla pratti : Tibiocillaria pratti : Ingura pratti : Carea pratti : Sphingiforma pratti : Risoba pratti : Zagira pratti.* Under the present method of notation, a state of complete uncertainty would arise if at any time any of these species was to be transferred from the genus in which it was described to some other genus, for it would be impossible to determine which was the species cited. For example, the notation " *Agrotis pratti* (Bethune-Baker) " could apply to any of the entities cited above.

It is to be noted that by the formula now suggested it would be possible to eliminate all cases of secondary homonymy, for in such a case, the nomenclatorial portion of the formula would differ in the two cases, although the systematic portion would be identical, as may be seen from the following example, in which for two species originally named *Phalaena nana* and *Noctua nana* respectively but both later transferred to the genus *Harmodia*, the formula for the first would be " *Harmodia nana* (*Phalaena* Hüfnagel) " and " *Harmodia nana* (*Noctua* Rottemburg) " respectively.

EXPLANATORY NOTES ON TERMS USED IN SECTION II

(1) I use the expression " name " to distinguish a unique term consisting of a single word, in contrast to the expression " denomination " which I use for a group of words applied to a systematic entity. The expression " species' denomination " is used to denote the phrase consisting of the generic name and specific name of a species; the expression " genus' denomination " is used to denote a generic name.

(2) I use the expression " designation " for any indication given for a systematic entity. It covers the expression " denominations " (see Note 1 above).

(3) I use the expression " validity " as signifying the presence of all those requisites which, under the Rules, are essential for the acceptance of a zoological name.

(4) By the expression " employment " I refer to the possibility of using a given name as a valid zoological name.

(5) This proposition is quite distinct from that given later (*I*, B) in Section II. In the latter place I have examined the case where a name, previously thought to be a *nomen nudum* or *nomen dubium* is stated to have an unequivocal content or where a name previously thought to be a valid name is on the contrary a *nomen nudum* or, owing to the doubt regarding its content, a *nomen dubium*, examination showing that the original author applied this name to an entity different from that previously believed. A particular case of this situation arises when it is found that a previously overlooked designation of the type species of a genus involves a change in the type species of that genus and therefore a change in its content.

(6) The situation of " Primary Homonymy " arises when the original denomination given to each of two systematic entities consists of the same term or terms. Examples: *Delta* and *Delta : Noctua lignosa* and *Noctua lignosa*.

(7) The situation of " Primary synonymy " arises when two different names (whether generic names or specific names) are published with the same content. In the case of generic names, this arises when two generic names are published, each having the same type species.

(8) There is a tendency to abandon the expression " genotype " to designate the type species of a genus, owing to the fact that this expression bears also a different, though more recent, meaning.

(9) This situation arises when a *nomen novum* is published in replacement of a name which is either invalid or unemployable. Such a name denotes exactly the same entity as that covered by the older name, as given to it by its original author. Where the older name is truly invalid, it must be discarded, but, if it is found to be valid and employable, it is to be retained, the substitute name, in that case, being discarded.

(10) According to the Code, names of specific rank include all those given to " species " or " subspecies." Many authors have excluded from this latter category the names applied to any category below the species, other than those given to the " subspecies " in the modern sense of a geographical entity. In my opinion, the expression " subspecies " should be interpreted in as a wide a sense as possible, in order, at least, to protect, for the purposes of priority, names which, through further investigation, may attain " specific " or " subspecific " rank, this being necessary, in order to avoid another possible cause for the changing of names.*

Editorial Note :* The point here made was met by the decision taken by the Fourteenth International Congress of Zoology at Paris in 1948, when a restricted definition was adopted for the expression " subspecific name " but at the same time a special, though separate, status was accorded for the purposes of priority to names given to entities belonging to all other infraspecific categories, means being at the same time provided for the promotion, where required, of " infra-subspecific names " to the rank of " specific name " or " subspecific name." See 1950 *Bull. zool. Nomencl.* **4 : 89–96.

DOCUMENT 1/21

Statement furnished by HOBART M. SMITH
(*University of Illinois, Department of Zoology, Urbana, Illinois, U.S.A.*)

Letter dated 14th March 1952

On the Question of Type Localities

I was very pleased to see the discussion accompanying your letter of 27th January. You have thoroughly set forth the difficulties and the logical solution for them. I am in complete agreement with your conclusions and method of approach, with only one reservation noted hereinafter. In addition a few modifications might be made here and there for the sake of clarity, depending upon your opinion of their necessity. These also are noted below. Dr. Edward H. Taylor has gone over the entire commentary, and his commentary is attached. He discussed the problems with a number of other systematists at the University of Kansas, and the commentaries of two are also attached. Dr. Wilson wrote his own, but I have transmitted in my own words the gist of the opinions of Drs. Taylor and Michener. They have not authorised the wording which is therefore only my impression of the news which they have expressed. My own comments follow.

I would suggest adding another paragraph between your Nos. 26 and 27, as follows :

> 26*a*. A similar situation which should be anticipated and governed by any new Rules is that of discovery, subsequent to designation of type locality under **23**(7) above, through attainment of further knowledge of geographic variation, that the type does not represent the population average at the locality formerly designated. A reasonable solution is an automatic one which permits correction of such former restrictions providing they can be shown to be sufficiently improbable that the type would not fall within the interquartile range of variation at the locality formerly designated. In such case the newly designated type locality should be so selected that the type would fall at least within the interquartile range of variation of the population at that locality. Uncertain cases, or those in which adoption of this procedure would lead to greater confusion than would some other course, should be submitted to the Commission for arbitration.

Secondly, I would recommend deletion of the last 11 lines of paragraph **28,** which would place what I believe would be an unnecessary and large burden upon the Commission, in favour of a substitute solution of more automatic nature, as follows :

> " . . . the type locality shall be determined by a system of precedence, the selection under each of the Rules of paragraph **23** having the preferential rank precisely of the order in which the rules are there listed. Thus a type locality determinable at any time by Rule **23***b* shall have immediate and permanent precedence over any selection under any of the subsequent Rules. Uncertain cases, or those in which adoption of this procedure would lead to greater confusion than would some other course, should be submitted to the Commission for arbitration."

Thirdly, would it not be well to add a paragraph making it clear that the Rules, as ultimately adopted, would be retroactive ?

In paragraph **23**(5) it might be well to make it clear that the conclusions there stated apply not only to localities doubtfully cited, but also to specimens doubtfully, or with any reservation, referred to the species. Locality data known at the time of original description, or unearthed at a later time, for such doubtfully referred specimens, should not be eligible for use in fixation of type locality for the species.

In paragraph **23**(6) it might be well to make it clear (1) that choice of a neotype or a lectotype automatically limits selection of a type locality, exactly as though it were an originally designated holotype ; and (2) that selection of a type locality automatically limits subsequent selection of a lectotype or neotype. Special cases should be submitted to the Commission for consideration.

Annexe 1 to Professor Hobart M. Smith's letter

Commentary by Dr. EDWARD H. TAYLOR (paraphrased)

On the whole agrees with the proposals made. They do not, however, clearly indicate proper procedure for two situations recently encountered, as follows :

(1) A given species has " Guatemala " as the locality. A reviser restricts it to Tomulco, Guatemala. The original expedition can be shown not to have passed near that locality, although positive records for the species are known from there. Is the reviser's choice acceptable ?

(2) A manuscript description of a new Mexican species is written by Dugès and sent to a colleague in Paris. This colleague has specimens from another locality of that species, from which drawings are made and published. Some years later the Dugès description is published and attributed to Dugès, but actually Dugès has never seen the specimens that served as the original " indication " of the species. What is the type locality—that of Dugès' specimens which served for the description, or that of the specimens serving as the original " indication " ?

Annexe 2 to Professor Hobart M. Smith's letter

Commentary by Dr. CHARLES D. MICHENER (paraphrased)

In general in agreement with the proposals made, but believes that too much work would be placed upon the International Commission. Wherever possible the Rules should allow the worker to make the decisions, as a matter of time-economy.

Annexe 3 to Professor Hobart M. Smith's letter

Statement by R. W. WILSON
(*University of Kansas, Lawrence, Kansas, U.S.A.*)

Fixing of Geologic Age and Position within Section

I do not think it is practical to set rules in respect of geologic age for nomenclatorial purposes. Such rules would aid on occasion, but the net result would be to cause more troubles than they would alleviate—particularly so if the age for nomenclatorial purposes were to be established by methods used for the selection of types.

The time scale was developed to more or less its present structure in the 19th Century concomitantly with the naming of thousands of fossil species. New divisions were added as the need seemed to arise, and these modifications extended well into the present century, if indeed, modification is not still in process. One result of the historical growth of the time scale is that the geological ages assigned to many species by the namers are not now acceptable, and in addition there were the usual number of outright errors in age assignment. As an example of the former situation what we now call Oligocene was at one time called early Miocene. As an example of the latter, the Etchegoin formation of California was originally dated as Miocene (species from this formation named *X-miocenicus* attest to this fact), but is now regarded as of Pliocene age, possibly in part early Pleistocene. Even at present, for example in mammalian paleontology, age determinations based on standard time units (e.g., late Miocene, early Pliocene, late Pliocene, early Pleistocene) are frequently so ambiguous that most American mammalian paleontologists make use of a system of provincial ages rather than the " international " time divisions (Eocene, Oligocene, etc.) of the Cenozoic.

Certainly if the proposed rules were to be used at all, the Commission would be passing judgment on hundreds of fossil species. I feel rather certain that most mammalian paleontologists would simply ignore such rules rather than appeal to the Commission for a change in " nomenclatorial " age. In this connection, perhaps it should be pointed out that many of the geological surveys (state and national) exercise rather tight control of the dates assigned to certain levels. Age assignments of strata may differ from one such organisation to another. I doubt that the surveys would be willing to assign their " rights " to a non-geological commission.

A fairly concrete example illustrates some of the difficulties if selection of age were made as suggested.

Ernestokokenia patagonica (Ameghino) : Cretaceous (Cenomonian) according to Ameghino.

No member of the Order to which *Ernestokokenia* belongs is Cretaceous in age. In fact no known Cretaceous mammal remotely approaches members of this genus in structure.

Ernestokokenia yirunhor Simpson is of late Paleocene age according to its namer. It is a smaller animal with simpler molar patterns than Ameghino's species. *E. patagonica* is actually slightly younger (early Eocene) rather than considerably older.

Ernestokokenia is a South American genus. Except for relatively recently named species, virtually the entire pre-Recent fauna of South America would have similar problems. Aside from formal dating, in some cases the stratigraphic sequence in South America is agreed upon, in others it is not—thus presenting additional problems for the Commission.

R. W. WILSON.

DOCUMENT 1/22

By H. ENGEL

(*Zoologisch Museum, Amsterdam, The Netherlands*)

Extract from a letter dated 18th March 1952

I have received the latest Parts of the *Bulletin* (Vol. **7**, Pts. 1-6). You have put forward the questions admirably. I think that the best way will be to complete the *Official Lists* of names as fast and as far as possible, as you propose.

DOCUMENT 1/23

By L. B. HOLTHUIS

(*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*)

Letter dated 15th April 1952

I consider that the stabilization of zoological nomenclature will be best promoted by means of amplifications, clarifications and extensions as proposed in 9-30 of your (1952, *Bull. zool. Nomencl.* **7** (5/6) : 148-188) paper and by the more extended use of *Official Lists.*

DOCUMENT 1/24

By G. H. E. HOPKINS, O.B.E., M.A.
(*British Museum (Natural History), Zoological Museum, Tring, Herts, England*)

Letter dated 30th April 1952

Commission's Reference Z.N.(S.)359

In *Bull. zool. Nomencl.* **7** : 187 and 188 you ask for replies from specialists to specific questions. Here are my replies :—

(1) I am opposed to an attempt to stabilize zoological nomenclature by the adoption of any novel expedient such as a " Law of Prescription ", partly because I think such a law would prove almost unworkable in practice, and partly because I think the aim is more easily attainable by the less drastic methods mentioned in paras. 9–37 of your article.

(2) I do not think that the disadvantages and difficulties can be adequately foreseen and provided against, and therefore have no suggestions for dealing with them.

(3) I would have thought certain of the suggestions enumerated in paras 9–37 of your article to be unnecessary, but there are none to which I object. I particularly welcome the suggestions in paras. 16 to 18 for the elimination of *nomina dubia*, both generic and specific, and that in para. 21 for the proposed recognition of neotypes.

(4) I have no additional proposals for promoting stability in nomenclature.

DOCUMENT 1/25

By FRANZ DANIEL

(*Zoologische Sammlung des Bayerischen Staates, Entomologische Abteilung, München, Germany*)

Letter dated 30th April 1952

Eine Bereinigung der zoologischen Nomenklatur ist in Weiterentwicklung der bestehendden " Regeln " nicht mehr durchführbar. Die von Herrn Professor Hering vorschlagenen Verbesserungsvorschläge müssen sich genau so totlaufen wie die bisherigen Anweisungen, da es praktisch unmöglich ist ein solches Gesetz wirklich eindeutig zu fassen. Eine erlösende Bereinigung kann nur durch eine radikale Abkehr von den bisher beschrittenen Weg erreicht werden. Dies mag hart klingen, wird sich aber eines Tages doch als einziger Ausweg durchsetzen. Je früher dies eingesehen wird, um so eher wird die Last von allen Beteiligten genommen einen wesentlichen Teil ihrer Lebensarbeit mit unfruchtbaren Auseinandersetzungen über Nomenklaturfragen auszufüllen.

Als möglicher Weg erscheint mir in ganz grossen Zügen folgende Verfahrensweise :

Ein IZK wählt eine Kommission für jede einzelne Tiergruppe, die innerhalb ihres Arbeitsbereichs eine nomenklatorische Unterteilung bis zur Familie vornimmt. Sie ist hierbei weder an Prioritätsregeln, noch andere einschränkende Bestimmungen gebunden, soll aber im Rahmen ihres subjektiven Ermessens bisher eingeführte Benennungen nach Möglichkeit übernehmen. Sie legt für die von ihr vorschlagenen Einheiten genaue Diagnosen, sowie die Typen höherer Ordnung fest. Diese Vorschläge sind von einem IZK zu prüfen und bestätigen, womit sie für die Zukunft als alleinige Grundlage dienen.

Von Tage der Anerkennung an hat die Neuaufstellung, Teilung oder Verschmelzung solcher Kategorien, sowie eine Anderung oder Erwieterung der festgelegten Diagnosen nur mehr Giltigkeit, wenn sie von einer für jede Tiergruppe permanent aufgestellten internationalen Kommission anerkennt wurde. Der Veröffentlichung ist die Genehmigungsnummer beizufügen, anderenfalls ihr die internationale Anerkennung zu verweigern ist. Alle zoologischen Fachzeitschriften sind anzuhalten nicht genehmigten Anderungen die Veröffentlichung zu verweigern. Gegen einen negativen Bescheid steht jedem Autoren ein Beschwerderecht zur nächsten IZK zu.

Dieser Beschneidung der Freiheit wissentschaftlicher Forschung müssen sich alle Kreise im Interesse einer gedeihlichen Zusammenarbeit fügen. Sie hat gewiss nichts mit Gewissenszwang in irgend einer Form zu tun.

Für die Bereiningung der Nomenklatur der Familien sind geeignete Bearbeiter zu ermitteln, die frei von allen Hemmungen durch den überlebten Prioritätsstandpukt die Benennung der unteren Kategorien bis zur Subspecies durchführen. Sie sind gleichfalls gehalten nach Möglichkeit eingeführte Bezeichnungen beizubehalten. In ihren Diagnosen gehen sie von dem erreichbaren Typenmaterial aus, soweit sie dieses jedoch nicht beschaffen können, oder falls der Typus durch mangelnde Voraussetzungen (schlecht

Bezettelung, Alterserscheinung etc.) nicht mehr voll auswertbar ist, bestimmen sie nach freiem Ermessen einen neuen Typus. Sämtliche verwendeten Typen sind als " Typus neuer Art " zu bezetteln und soweit nötig nach modernen Gesichtspunkten unter Anwendung einer *Differenzialdiagnose* zu beschreiben. Ebenso ist, falls nicht gute Bilder verwiesen werden kann, in denjenigen Gruppen, bei denen habituelle Gesamteindruck durch die Sprache nicht vermittelt werden kann (Lepidoptera !) der Typus abzubilden. Genitaldiagnosen ist grosse Bedeutung einzuräumen, jedoch ist die in einigen Ländern eingerissene Unsitte *nur* nach Genitalmerkmalen zu urteilen und den Koppulations-Apparat allein abzubilden wenigstens bei Lepidoptera tunlichst zu vermeiden. Die Typenform ist sowohl ihrer genauesten Heimat nach, wie in Bezug auf ihr Aussehen innerhalb der modifikatorischen Möglichkeiten festzulegen. Benennungen von regelmässig wiederkehrenden Aberrationen sind weiterhin von internationalen Bindungen freizuhalten, es ist aber tunlichst darauf hinzuarbeiten für grössere Gruppen durch Aufstellung von Collektivbezeichnungen eine vereinfachte Verständigung zu erreichen. Alle nicht mehr sicher aufklärbaren Beschreibungen, bei denen aber auf Grund der vorhandenen Unterlagen eine Einreihung als Synonym nicht ratsam erscheint, sind als sp. dubiosa aufzuführen. Bei späterer Klärung des Falles sind sie neu zu beschreiben, wobei der damit Betraute als Autor zu gelten hat. Der bisher fraglich geführte Name wird dabei zum Synonym.

Alle von den Familien Bearbeitern als Synonyme angesprochenen Bezeichnungen scheiden endgiltig aus, auch wenn sich später herausstellen sollte, dass diese Ansicht unrichtig war. Ebenso sind alle von den Familien Bearbeitern etwa übersehene Namengebungen automatisch gestrichen. Für beide Fälle ist gegebenfalls eine Neubeschreibung unabhängig von den bisherigen Literaturangaben, durchzuführen.

Alle diese Vorschläge sind vom Standpunkt der Entomologie aus gesehen, wo die Verhältnisse durch den ungeheueren Formenreichtum völlig anders liegen als in den übrigen Gebieten der Zoologie. Es wäre deshalb zu erwägen ob nicht eine internationale Vereinbarung möglich wäre, die einen Teil (ich denke hier vor allem an die Entscheidung von der Familie abwärts) der notwendigen Regelungen im Insekten Bereich dem IEK übertragen würde.

Ich bin mir bewusst, dass die hier in ganz groben Umrissen niedergelegten Vorschläge heute noch keinerlei Aussicht auf Erfolg haben. Ebensosehr glaube ich aberauch unbedingt, dass die von Prof. Dr. Hering angeregten Verbesserungs-Wünsche zu keiner Bereinigung der chaotischen Zustände unseres systematischen Gerippes führen können. Dazu gibt es meines Erachtens keinen anderen Weg als den einer völlig neuen Grundlage. Je eher der Entschluss hierzu gefasst wird umso leichter wird diese Umstellung sein. Ich denke hierbei besonders auch an das Ansehen des Systematikers, welches in den weiten Kreisen derjenigen, die dessen Erkentnisse nur als Arbeitsunterlage benutzen, ständig sinkt—und sinken muss, da ihnen ein stets schwankendes Gerüst geboten wird, dass sie nicht mehr als internationales Verständigungsmittel anerkennen können. Mit Massnahmen, die lediglich auf eine Verbesserung nicht mehr tragbarer Grundlagen hinauslaufen ist der Krankheitsherd nicht mehr zu heilen.

DOCUMENT 1/26

By FRIEDRICH KÜHLHORN

(*Museum, München, Germany*)

Statement dated 2nd May 1952 communicated by Professor E. M. Hering

Editorial Note: The present communication from Dr. Kühlhorn and also the immediately following communications from Dr. Haltenorth (Document 1/27) and from Dr. Sheljuzhko (Document 1/28) were prepared by the specialists concerned by reference to a summary of the points raised in my paper of March 1952 (*Bull. zool. Nomencl.* **7** : 148–188) which Professor E. M. Hering kindly prepared for communication to German zoologists. Since Dr. Hering's summary bears numbers different from those used in the foregoing paper, the three comments in question are not intelligible without Dr. Hering's summary, which is accordingly reproduced as an annexe to the present document. (intl'd. F. H., 10th April 1953).

Stellungsnahme zu den vorschlagen zur neufassung Internationalen Nomenklaturregeln

Eine Neufassung der Regeln darf auf keinen Fall zu einer weiteren Komplizierung der zoologischen Nomenklatur führen, die trotz aller Verbesserunger immer noch genungend geschichtlich verständliche Schwächepunkte behalten werden.

Die Systematik darf nicht nur Selbstzweck sein, sondern sie muss bei der grossen wirtschaftlichen Bedeutung vieler Tiere auch die berechtigten Wünsche der Praktiker berücksichtigen, für die eine Vereinfachung eine ungeheure Erleichterung ihrer Arbeit bedeuten würde. Ich trete deshalb allein schon aus diesen Gründen für die *Beibehaltung des Prioritätsprinzips* ein, des en Schwächen durch die im Absatz VII/II vorgeschlagenen Erweiterungen weitgehend ausgeglichen werden könnten.

Annexe to Editorial Note to Document 1/26

Summary prepared by Professor E. M. Hering of the main points in regard to zoological nomenclature raised in the seven papers by Mr. Francis Hemming; papers published in 1952 in Parts 1–6 of volume 7 of the "Bulletin of Zoological Nomenclature."

Sieben Probleme der zoologischen Nomenklatur

die eine Neufassung der Internationalen Regeln notwendig erscheinen lassen, über die auf dem 14. Internationalen Kongress für Zoologie, Kopenhagen, 1953, entschieden werden soll.

In dem Zeitraum von 1901 bis zur Gegenwart hat sich die Fassung verschiedener Artikel der " Internationalen Regeln für die zoologische Nomenklatur " (nachfolgend als " Regeln " bezeichnet) als unzulänglich erwiesen.

Der Internationale Kongress für Zoologie Paris, 1948, beauftragte die Internationale Kommission für zoologische Literatur (nachfolgend als INK. abgekürst), Neufassungsvorschläge der betreffenden Artikel dem 14. Int. Kongr. f. Zoologie, Kopenhagen, 1953, vorzulegen, der über sie entscheiden und die Entscheidungen in die " Regeln " einkörpern soll.

Das in Paris, 1948, endgültig festgelegte Verfahren der Internationalen Kommission zur Ausübung der ihr vom Kongress Monaco, 1913, übertragenen Vollmachten zur fallweisen Suspendierung der " Regeln " (im folgenden kurz als " Verfahren " bezeichnet) besteht darin, dass.

(*a*) Ein oder mehrere Spezialisten stellen einen begründeten Antrag auf Klärung einer zweifelhaften Frage oder auf Aufhebung der Regeln in einem gewissen Falle, dass die INK. eine Entscheidung auf Grund ihrer Vollmachten treffen möge.

(*b*) Dieser Antrag muss im Wortlaut mit der Begründung veröffentlicht werden.

(*c*) Die auf diese Veröffentlichung eingehenden Stellungnahmen der Spezialisten werden ebenfalls veröffentlicht.

(*d*) Auf Grund dieser Stellungnahmen und sonstiger Befragung von zuständigen Spezialisten entscheidet die INK. regelverbindlich über den Fall, wobei sie Erwägungen über Stabilität und Uniformität den Vorrang gibt gegenüber philologischen Argumenten.

Ich habe die Behandlung dieser Probleme in vol. 7 des Bulletin of Zoological Nomenclature in möglichster Kürze darzustellen versucht, um eine Stellungnahme der deutschen Zoologen und Paläontologen ohne die grosse Mühe des eingehenden tudiums dieses Bandes des " Bulletin " zu ermöglichen. Wo ich mich nicht klar ausgedrückt habe, bin ich gern bereit, auf Anfrage nähere Mitteilungen zu machen.

Ich halte es für aussererdentlich wichtig, dass Spezialisten sichmög lichst zahlreich zu den zur Entscheidung kommenden Vorschlägen äussern, damit einmal keine Entscheidung in Bezug auf die Neufassung der " Regeln " getroffen wird, die den Wünschen der Mehrheit der Wissenschaftler widerspricht, zum andern die deutsche zoologische Wissenschaft ihre Existenz dokumentiert und aus ihrer Abseitsstellung herauskommt, die sich in gewissen Beschlüssen Int. Kongr. f. Zoologie zeigt (Paris, 1948 beschloss Ausschaltung der deutschen als offizieller Sprache für INK. und Veröffentlichungsverpflichtungsaufhebung für Ausserkraftsetzung von " Regeln " im " Zoologischen Anzeiger " die früher vorgeschen war). Abstellung solcher Mängel kann nur durch Aktivität und Beteiligung auch deutscher Wissenschaftler erreicht werden.

Da ich bis Ende Mai ein Referat über solche Stellungnahmen auszuarbeiten habe, wäre ich für recht baldige Stellungnahme des Stabes der Wissenschaftler des Museums—wie auch anderer befragter Spezialisten—zu den nachfolgend berichteten Vorschlägen für die Neufassung der " Regeln " zu Danke verpflichtet.

E. M. HERING,

deutsches Mitglied der Internationalen Kommission für Zoologische Nomenklatur.

VII. Vorschlage zur Erzielung grösstmoglicher Stabilität in der Nomenklatur

A. Die mangelhafte Stabilität zoologischer Namen hat sich in letzter Zeit zu einer grossen Erschwernis wissenschaftlicher Arbeit ausgewirkt, so dass an Massnahmen zur Erzielung einer *grösseren Stabilität* gedacht werden muss. Sie können bestehen entweder in der *Einführung ganz neuer Prinzipien* in die " Regeln," oder in der *Erweiterung und Klärung schon vorhandener Regeln*, oder im *erweiterten Gebrauch* der schon in den " Regeln " *vorhandenen Vorschriften.*

B. Vorschläge zur Besitigung der Instabilität:

I. *Einführung eines " Praescriptions-Prinzips "* (etwa dem " Konzinuitäts-prinzip von HEIKERTINGER entsprechend). Es würde besagen: Gattungs-oder Artnamen, die etwa vor 1850 veröffentlicht wurden und seitdem von keinem Autor mehr verwendet wurden, sollen zu verwerfen sein, wenn sie mit einem nach diesem Zeitpunkt dauernd gebauchten Namen in Konflikt kommen. (Andere Vorschläge schreiben anstelle eines festen Datums von " lüngerer Zeit " etc.).

1. Ist ein solches Prinzip durchführbar? Es würde in besonderen Fäller das Prioritäts-Gesetz unterdrücken, müsste auch unterscheiden, ob ein *Name vielleicht einmal*, in anderen Fällen *dauernd gebraucht* würde, der *Zeitpunkt* vom Beginn der Gültigkeit müsste *exact fornuliert* werden.

2. Schwächen des Prinzips: Es ist *unmöglich*, einen *negativen Begriff* (Nichtgebrauch) in die " Regeln " *aufsunchmen.* Der Nichtgebrauchsnachweis ab 1850 etwa erfordert ein *nicht*

durchführbares Studium der gesamten Literatur. Die Gewissheit zu erreichen, würde unvorstllbaren *Zeitverlust bei der wissenschaftlichen Arbeit* kosten. Überraschungen wie die kürzlich erfolgte Auffindung einer Schrift von Linnaeus, die bisher unbekannt gewesen ist, könnten sich auch in Bezug auf spätere Literatur erwarten lassen.

II. Stabilität durch *Erklärung und Erweiterung der* schon vorhandenen "*Regeln*"! Wo eine die Stabilität gefährdende Unklarheit in den Regeln besteht, ist eine Entscheidung durch Antrag an die INK. zu erzielen.

a. *Erklärungen.*

1. Die Regeln sollen eine Präambel erhalten, die die Grundprinzipien enthält, unter denen die nachfolgenden Artikel angewendet werden sollen.

2. Strittige Begriffe in den Vorschriften der "Regeln" sollen durch aus führlichere Vorschriften geklärt werden (cf. I. Problem: Emendation).

3. Die der INK. gegebenen Vollmachten (Monaco 1913, Paris 1948) geben jede Möglichkeit, Vorschriften der "Regeln" in Bezug auf einzelne Fälle aufzuheben, um Stabilität zu erhalten.

4. Wo mehrfache Interpretation eine Artikels der "Regeln" möglich ist, kann immer Entscheidung durch Antrag an die INK. erfolgen.

5. In allen anderen Fällen, wo unerwünschter, die Stabilität gefährdender Namenwechsel droht, kann durch Antrag an die INK. eine zugunsten der Stabilität ausfallende Entscheidung erwirkt werden.

6. Notwendig ist analog den bereits in den "Regeln" verankerten "Offiziellen Listen" ein "Offizieller Index" der zu verwefenden Bücher aufzustellen (die entweder nicht der binominalen Nomenklatur folgen oder anderweitig die Stabilität gefährden). Die Überführung auf diese Liste soll nach üblichem Verfahren durch Vollmacht der INK. übertragen werden, die in den "Regeln" zu verankern ist.

7. Zweifelhafte Typenbestimmung für Genera kann durch die INK! auf Antrag festgelegt werden (Lumbricus L. Typus terrestris L., terrestris ist Nomen dubium!)

8. Artnamen, die Nomina dubia sind, können nach üblichem Verfahren durch die INK. auf den Bereits vorhandenen "Index der verworfenen Namen" gesetzt werden. Notwendigkeit ergibt sich aus späterer Deutung früherer Nomina dubia (cf. STRESEMANN über die Gmelin-1789-Namen!).

b. *Erweiterung der Regeln.*

1. Begriffsfestlegung für "Typus" in Art. 31 der "Regeln" (Kongress Paris 1948) durch Anrekennung und Definition für Holo-, Syn- und Lektotypus, sowie Prozedur und Bedingungen für Schaffung von Lektotypen.

2. Dir Pariser Regelung lässt Typenfestlegung unklar in Fällen, wo kein typisches Material mehr existiert und macht keine bindenden Vorschirften über nominotypische Subspecies, wenn ursprüngliche Fundortangabe zu unbestimmt ist. Hierfür sind ausführliche Vorschriften notwendig.

3. Einfügung des "Neotypus" (cf. VI. Problem).

4. Vorschriften über die typische Lokalität polytoper Artnamen, deren Fehlen zur Vieldeutigkeit bei der Aufstellung der nominotypischen Unterart geführt hat. Deswegen sind in Art. 30, 31 der "Regeln" einzufügen folgende Vorschläge:

   a. Wo der Autor selbst die Typenlokalität bezeichnet hat (wenn er sie nicht selbst als zweifelhaft bezeichnet) gilt dies auch für die nominotypische Unterart.

   b. Der am Holo-bezw-Lektotypus angebrachte Fundertzettel ist, wenn genauer als in der Beschreibung, für die Aufstellung der nominotypischen Unterart massgeblich, wenn vom gleichen oder späteren Autor spater veröffentlicht.

   c. Wo Art-oder Unterartname eine Lokalität bezeichnet, ist diese als topotypisch anzusehen, wenn Patria-Angabe bei der Beschreibung fehlt.

   d. Wenn ein Art-oder Unterartname für ungültig erklärt und durch einen neuen (oder ein Synonym) ersetzt wird, soll der Ersatzname gleichen Topotypus besitzen.

   e. Wo die Lokalität nicht genauer in der Originalbeschreibung präzisiert wurde und auch nach (*b*) nicht zu erhalten ist, ist Auswahl durch gleichen oder späteren Autor später massgeblich. Weitere Eestriktion der Lokulität erfelgt durch die folgenden Autoren.

   f. Wo Patria-Angabe bei der Beschreibung fehlt, aber Hinweis auf frühere Veröffentlichung des Autors angegeben ist, wird die dort angegebene Lokalität als topotypisch angesehen und geht den Lokalitäten in anderen gleichzeitig angegebenen Referenzen auf andere Autoren vor.

g. Wo ursprüngliche Fundortangabe irrig ist oder fehlt, kann auf Antrag die INK. den Topotypus für Art oder Unterart festlegen. Wenn nach solcher Festlegung vermisste Holo-oder Syntypen wieder auftauchen, die die Lokalität in anderem Sinne klären, ist Entscheidung durch die INK. zu beantragen.

5. Bei Fossilien ist geologisches Alter und Horizont wichtiger als die Lokalität, deshalb sollen die Vorschriften einen Zusatz erhalten, dass für erste analog zu vorigen Regeln zu verfahren ist.

6. Bei Parasiten ist analoger Zusatz in Bezug auf den Wirt nötig.

III. Erweiterter *Gebrauch* schon *bestehender Vorschriften* zur Erhöhung der Stabilität.

Der Kongress von Monaco, 1913, schuf die " Offizielle Liste " der Genusnamen (für Nomina conservanda), von der bisher zu wenig Gebrauch gemacht wurde (seitdem etwa 600 Namen eingefügt !). Der Kongress von Paris, 1948, legte das Verfahren für die INK. fest und schuf die " Offizielle Liste " auch für die Artnamen, sowie den " Offiziellen Index " der zu verwerfenden Namen. Dir Listen sind jetzt in den " Regeln " verankert, das liberum Veto in der Kommission wurde beseitigt. Diese Listen geben jederzeit die Möglichkeit durch Antrag an die INK. Namensänderungen, die die Stabilität gefährden, auszuschalten.

Fragen an die Spezialisten

1. Kann Stabilität besser erreicht werden durch ein " Praeskriptionsgesetz oder durch Ausbau des Systems der " Regeln "?—Wenn im Sinne der Praeskription : wie können die Schwächen nach B.I.2. beseitigt werden?—Wenn im Sinn des Ausbaues der " Regeln " : Ist man für die obigen Vorschläge, welche weiteren Vorschläge zu machen ?

DOCUMENT 1/27

By Th. HALTENORTH

(*Museum, München, Germany*)

Statement, dated 14th May 1952, communicated by Professor E. M. Hering

Editorial Note: For an explanation of the reference cited by Dr. Haltenorth in the following note, see Editorial Note to Document 1/26 and the document prepared by Professor Hering annexed thereto.

VII. B.II sollte bevorzugt werden, d.h. Namens-Stabilität durch Ausbau der Regeln, die bereits weitgehend für Stabilität sorgen und nach denen schon Jahrzehnte gearbeitet wird.

DOCUMENT 1/28

By LEO SHELJUZHKO
(*Zoologische Sammlung des Bayerischen Staates, München, Germany*)

(1) Statement, dated 16th May 1952, communicated by Professor E. M. Hering

Editorial Note: For an explanation of the reference cited by Dr. Sheljuzhko in the following note, see Editorial Note to Document 1/26 and the document prepared by Professor Hering annexed thereto.

VII. " Einfuhrung eines Praeskriptions-Prinzips "—möchte ich als kaum durchführbar und unzulässig verwerfen. Dagegen scheint es mir, dass die Stabilität durch Erklärung und Erweiterung der schon vorhandenen Regeln allmählich gute Fortschritte erzielen kann.

Zu dem Penkt 4d der " Erweiterung der Regel " wäre zu bemerken, dass dieser unerfüllbar ist. Ein Synonym ist ein Name, der auf bestimmtem Material basiert ist, das aus einem bestimmten Fundort stammt. Sollte solch eine synonyme Bezeichnung aus irgend-welchen Gründen Gultigkeit bekommen und den früher gebrauchten Namen ersetzen, so bleiben doch die Typen der beiden Formen mit ihren Fundorten bestehen und können diese Fundorte nicht geändert werden. Ganz anders verhält es sich, falls es sich um einen nomen novum handelt ; dieser nom. nov. soll überhaupt keine eigene Typen besitzen, d.h., dass als Typen des nom. nov. diejenigen der Form gelten, für die er den Ersatznamen bildet.

(2) Letter dated 27th March 1953

Bei einem Ersatz eines Art—oder Unterartnamens kenn es sich um zwei recht vershiedene Fülle handeln.

Im ersten Fall ist der Name nur ein *novem novum*, der als Ersatzname für die frühere Bezeichnung geschaffen wurde. Solche *nomina nova* sollten überhaupt keine Typen und müssten also solche diejenigen das alten Namens gelten.

Ganz andere verhält sich die Sache falls an die Stelle des alten Namen ein Synonym trit. Diese Synonymy wurde bei ihrer Aufstellung auf bestimmtes Material gefründet, d.h., dass sie auchihre Typen aus bestimmten Fundorten besitzen. Es wäre ein reiner Zufall, wann diese Typen aus den selben Orte stammen sollten, von wo die Typen der Form stammten, für die die synonyme Bezeichnung eintreten soll. Nun glaube ich, dass es nicht möglich wäre zu fordern, dass der in diesem Fall der Ersatzname den gleichen Topotypus hat, wie es im Punkt " 4.d " gefordert wird.

DOCUMENT 1/29

By W. J. ARKELL, M.A., D.Sc., F.R.S.
(*Sedgwick Museum, Cambridge University, Cambridge*)

Letter dated 23rd May 1952

I have read with admiration your most thorough and clear papers in the *Bull. zool. Nomencl.* Vol. **7**, Parts 1-6.

I enclose comments on Z.N.(S.)359 (stability). I should be content to see everything not covered by my comments go through as expounded in your papers.

Enclosure to Dr. W. J. Arkell's letter of 23rd May 1952

I am not in favour of a Law of Prescription. In my opinion it would cause far more confusion than it would be worth, and the objects desired can be achieved by sensible behaviour of specialists in refusing to be hustled into unnecessary changes until they have submitted the case to the Commission and invoked the Plenary Powers.

My recent applications on *Pachyceras*, *Sphaeroceras*, *Pecten asper*, *Exogyra virgula*, etc., illustrate the kind of action to be encouraged before sweeping changes are accepted under the Rule of Priority.

In the past, authors have not had the chance to use the existing machinery in this way, because of the extreme slowness of publication and processing of applications. If one was ever going to publish any work in a lifetime, one simply had to ignore the Commission and follow the letter of the existing Rules. (As you know, one waited 10-15 years for a case to be dealt with.) Now that you have got all that changed, I consider it should be made widely known by a Recommendation that it is the duty of an author to investigate possibilities of adjustment under the Plenary Powers before rushing into changes of name as the result of " bibliographical discovery "—i.e., reading Neave's *Nomenclator*.

DOCUMENT 1/30

By J. R. DYMOND
(*Department of Zoology, University of Toronto, Canada*)

Statement received 27th May 1952

I consider that the stabilization of zoological nomenclature would best be promoted by means of amplifications, clarifications and extensions carried out within the existing framework of the *Règles* and by the more extended use of the existing provisions for stabilizing individual names through the *Official Lists*.

DOCUMENT 1/31

By CHARLES H. BLAKE

(*Massachusetts Institute of Technology, Department of Biology, Cambridge, Massachusetts, U.S.A.*)

Enclosure to a letter dated 5th June 1952

Proposal of a Nomenclatorial Statute of Limitation

Preamble.—In the interest of nomenclatorial stability a statute of limitation should be enacted as set forth below. It has been the general experience of those peoples living under systems of elaborate statute laws that more harm than good flows from permitting actions to be initiated indefinitely long after the cause of action comes into existence. It has also become apparent that the unrestricted application of the Rules of Priority and Homonymy has not led to nomenclatorial stability.

The following rule or statute shall come into force six months after its enactment by the International Zoological Congress.

Section 1. *Definitions.*—Terms used in this statute shall have the definitions given in paragraphs (*a*) to (*e*) of this section.

(*a*) The actual zoological organism or concept bearing a name is an onomatophore. It acquires prescriptive right to a name by de facto, published assignment of the name to the given onomatophore. Its prescriptive right to a name is extinguished by non-application of the name within the term of this statute.

(*b*) A *nomen conservatum* is a name which has been placed on the " List of Official Names " by action of the International Commission on Zoological Nomenclature (hereafter, the Commission).

(*c*) A *nomen conservandum* is a name belonging to either of the following categories :—

(1) The name of a genus that is the type of a supergeneric category.

(2) Any name which can qualify for the benefit of this statute and which has not been the subject of a final determination by the Commission.

(*d*) Contested application is the application of a name to more than one onomatophore. Such application shall be deemed contested as regards all of the onomatophores.

(*e*) Primary zoological literature is all published work in which the authors could have contested the applications of the names used. For the purposes of this statute literature shall be deemed primary unless there is conclusive evidence to the contrary.

Section 2. A name shall be entitled to the benefit of this statute regardless of its original rank or of the rank subsequently or currently assigned to it.

Section 3. The benefit of this statute must be expressly claimed.

Section 4. The provisions of this statute apply to all cases of priority, synonymy, and homonymy, except as provided otherwise. (Sections 3, 8, 9 and 10).

Section 5. The provisions of this statute apply to all cases of the designation of types of genera or their subdivisions or of higher rank than genera, except as provided otherwise. (Sections 3, 8, 9 and 10).

Section 6. Exclusive and uncontested possession of a name by an onomatophore for fifty years or more shall be deemed conclusive as to the correct application of the name provided, however, that the given application shall have been made on three different occasions in primary zoological literature within the period of fifty years.

Section 7. Exclusive and uncontested possession of a name by an onomatophore for seventy years or more shall be deemed conclusive as to the correct application of the name even though it shall not have been applied on three different occasions within the period of seventy years.

Section 8. The doubtful application of a name to an onomatophore or a doubtful type designation shall be deemed non-application for the purposes of this statute.

Section 9. The provisions of this statute shall not apply during any time when fraudulent concealment of the facts shall be adjudged to have occurred.

Section 10. This statute shall not apply to cases where action which could have been challenged under this statute has been taken prior to the coming into force of this statute.

DOCUMENT 1/32

By S. G. KIRIAKOFF, L.Sc.
(*Zoological Museum, Ghent University, Ghent, Belgium*)

Enclosure to a letter dated 5th June 1952

MEANS TO BE FOUND FOR PROMOTING THE GREATEST POSSIBLE STABILITY IN ZOOLOGICAL NOMENCLATURE

(*Bull. zool. Nomencl.* **7,** Pts. 5-6, 148-188)

Preliminary remark.—I am fundamentally against any restriction of the Law of Priority. In my opinion, any case of suspension of the *Règles* in favour of a more recent name on the ground that the latter has been in general use, is a solution of facility and is, moreover, nothing less than an encouragement either of (former) mistake or of ignorance. The first describer of a form has, I believe, the (mostly posthumous) moral right to have the name he has given used in preference to any younger name regardless of the fact that the latter has been in common and general use for ever so long.

The above is, however, only a declaration of principle, and, as it seems that the great majority of the zoologists (although most of the latter are physiologists, ecologists, biochemists and so on, who are not interested in Taxonomy and who often do not have a clear idea of the objects and of the meaning of the latter) are against the unrestricted use of the Law of Priority, I am quite willing to help in promoting the stability in nomenclature they are in such a hurry to find.

Answers to the questions l.c., p. 187-188 :—

(1) I consider that a " Law of Prescription " such as it has been proposed by some zoologists (par. 4-7) would be unable to achieve any serious purpose ; for those impatient to preclude any possibility of further name changing, a means is suggested below (vide (2)). But personally I am in favour of gradually achieving the stability so desired by means of amplifications and so on, within the existing framework of the *Règles*, as suggested in para. 9-37.

(2) The radical means suggested to achieve at once the stability in nomenclature is the following :—

An article is to be inserted in the *Règles* to the effect that—

(a) no further changes of generic and specific names will be accepted ;

(b) that any paper or book published before the date of publication of the present article in the *Bull. zool. Nomencl.* and containing names of genera and/or species generally known under other younger names, and consequently apt to supersede the latter, is to be considered as non-existent for nomenclatorial purposes.

Of course, the provision under (b) would apply in fact only to the (relatively few) old papers or books that have remained unknown for perhaps a century or longer, and that are being periodically unearthed by people (*bona fide* or otherwise) with as result necessary but, to many workers, irritating changes in nomenclature.

The means suggested above makes it unnecessary to make up the proposed list of invalid publications.

(3) I fully agree with the various suggestions enumerated in para. **9-37** of your inquiry.

(4) See under (2).

DOCUMENT 1/33

By ERICK M. HERING

(*Abteilungsleiter am Zoologischen Museum der Humboldt-Universität, Berlin*)

Statement received on 7th June 1952

Stellungnahme zu den Vorschlägen zur Sicherung der Stabilität in der zoologischen Nomenklatur

Das in den Para. **4–7** im *Bull. zool. Nomencl.* vol. 7 behandelte " Praeskriptions-Prinzip " wird nach Mitteilung von Dr. Walter Forster von der Zoologischen Sammlungs des Bayerischen Staates mit 11 von 12 Stimmen des Zoologen-Stabes des Museums München unbedingt *abgelehnt*. Die Vorschläge zur Sicherung der Stabilität durch Erklärung und Erweiterung der " *Règles* " werden *unterstützt*.

Der Zoologen-Stab des Museums Berlin lehnt das Praeskriptions-Prinzip ebenfalls ab und *unterstützt* die Vorschläge zur Sicherung der Stabilität durch Erklärung und Erweiterung der " *Règles* " mit folgenden Ausnahmen :

Zu Para. **18** (p. 168–170) : Der Vorschlag, dass *nomina dubia* auf Antrag der Spezialisten durch die International Commission on Zoological Nomenclature auf den " Offiziellen Index der zu verwerfenden Namen " gesetzt werden können, wird mit 13 von 14 Stimmen *abgelehnt*.

Zu Par. **23** (4) wird für den Vorschlag der Zusatz für nötig gehalten : " wen die Spezies oder Subspezies an dem Ort vorkommt oder vorkam, den der Name bezeichnet."

DOCUMENT 1/34

Statement of the views of the NOMEMCLATURE COMMITTEE established jointly by the DEUTSCHE ZOOLOGISCHE GESELLSCHAFT, the PALAONTOLOGISCHE GESELLSCHAFT and the SENCKENBERGISCHE NATURFORSCHENDE GESELLSCHAFT

Editorial Note : The following document which was published in the issue of 15th June 1952 of the serial publication *Senckenbergiana* (**33**(1/3) : 193–196), deals with a number of questions, of which some only are directly concerned with the problem whether provisions and, if so, what provisions should be inserted in the *Règles* for the purpose of promoting stability in zoological nomenclature. It is published here *in extenso*, since in the circumstances this appears to be the most appropriate course, having regard to the fact that the authors of this document base all their arguments upon the need for promoting stability in zoological nomenclature. At later points in the present volume backward references will be made to those portions of the present document which refer to subjects which form separate items on the Agenda for the Copenhagen Meetings, in order thereby to obviate the risk of the views expressed in regard to those matters in the present document from being overlooked by reason of not having been submitted in the form of separate statements (intl'd) F.H. 10th April 1953.

Antrag an die Internationale Kommission fur Zoologische Nomenklatur

Die unterzeichneten Gesellschaften haben zur Prüfung der laufenden Fragen eine Nomenklatur-Kommission eigesetzt und diese zu folgender Erklärung ermächtigt.

A. Aufschub jeder Änderung der Regeln bis nach dem Kongress von 1953

Die auf dem Zoologen-Kongress in Paris 1948 gefassten Beschlüsse zur Änderung der geltenden Internationalen Regeln der Zoologischen Nomenklatur sind, vor allem in Nord-Amerika, auf den Widerspruch vieler Forscher und grosser Körperschaften gestossen[1]). Die formellen Einwände richten sich gegen die Unrechtmässigkeit des Verfahrens, besonders gegen die ungenügende Vorbereitung und das nicht einwandfreie Zustandekommen der Beschlüsse.

[1]Veröffentlicht sind besonders scharfe Proteste der Nomenclature Discussion Group in " Science," **110** : 594, 1949 und **112** : 27-30, 1950. Diese Gruppe besteht aus 71 Zoologen (Neo- und Paläozoologen) folgender Anstalten in Washington : Smithsonian Institution, U.S. National Museum, Paleontological Branch of the U.S. Geological Survey, Bureau of Entomology of the U.S. Dep. of Agriculture, Fish and Wildlife Service of the U.S. Dep. of the Interior, U.S. Public Health Service. Ja sogar der Präsident der Internationalen Nomenklatur-Kommission selber, der in Paris nicht anwesende Dr. J. L. Peters-Cambridge (Mass.), hat sich diesem Protest gegen die Pariser Beschlüsse seiner Kommission (d. h. der dort in Erscheinung getretenen Kommission) angeschlossen und hat den unterzeichneten Vertreter der Paläontologischen Gesellschaft brieflich zu einem ähnlichen Vorgehen aufgefordert.

Für wertvolle Anregungen danken wir Dr. R. E. Blackwelder (Secretary der Nomenclature Discussion Group, Washington) und Dr. E. Mayr (American Museum of Natural History, New York).

Die sachlichen Einwände betonen die Beunruhigung der Nomenklatur durch die drohende und unnötige Änderung sehr vieler Namen.

Die Zoologen (neontologische und paläontologische) ganzer Länder sind zu dem "Internationalen" Kongress in Paris 1948 weder eingeladen noch zugelassen worden. Sie dürfen erwarten, dass ihnen ausreichende Gelegenheit zur persönlichen Beteiligung an allen Beratungen und Beschlüssen gegeben wird, die eine internationale Anerkennung finden sollen.

Erst der Internationale Zoologen-Kongress in Kopenhagen 1953 wird diese Möglichkeit bieten. Vorgreifende Entscheidungen würden die Gefahr eines Schismas der Nomenklatur heraufbeschwören.

B. Sachliche Einsprüche gegen die Pariser Beschlüsse von 1948

Die Pariser Beschlüsse widersprechen in vielen Punkten dem obersten Grundsatz der Nomenklatur, nämlich dem Prinzip der Stabilität. Ohne damit die übrigen zu billigen, wird hier gegen folgende Punkte Einspruch erhoben:

1. Allgemein: Neue Bestimmungen sollen mit rückwirkender Kraft eingeführt werden, z.B. bei der Zeilen-Priorität, bei Emendationen und in einer Reihe weiterer Fälle. Es sollen also veröffentlichte und bisher gebrauchte Namen nachträglich geändert werden, wenn sie den neuen Bestimmungen nicht entsprechen. Das wäre gegen den Sinn und die gesunde Entwicklung der Nomenklatur zur Stabilität.
2. Die private Veränderung veröffentlichter Namen aus nicht-zoologischen Gründen soll, statt verhindert, begünstigt werden. Eine Reihe von neuen Bestimmungen fordert zu "Emendationen" aus philologischen und ähnlichen Erwägungen geradezu heraus. Das Prinzip der Stabilität verlangt dagegen, dass ein Name in der veröffentlichten Form grundsätzlich unantastbar bleibt[2]). Ist in einem bestimmten Falle die Änderung der Form eines Namens begründet, dann kann nur die Kommission eine endgültige und obligatorische Emendation vollziehen. Die Erlaubnis oder gar die Verpflichtung zu privaten Emendationen, die oft in mehr als einer Weise möglich sind, führt zu einer dauernden Beunruhigung der Namen.
3. Die Priorität soll verschärft werden zu einer "Seiten-Priorität, Zeilen-Priorität und Stellung-in-der-Zeile-Priorität." Es soll also, bei einer taxonomisch nötigen Vereinigung von Gattungen oder Arten, der Revisor nicht mehr denjenigen Namen konservieren dürfen, der sich durch Gebräuchlichkeit oder Sicherheit der Unterlagen am besten für die Wissenschaft eignet. Vielmehr soll der Revisor gezwungen werden, dén Namen zu wählen, der in der betreffenden Veröffentlichung durch Zufall an erster Stelle gedruckt worden ist. Damit wird das Prioritäts-Prinzip, das doch nur als Hilfsmittel für die Stabilität eingeführt worden ist, missverstanden und wird der Stabilität als Selbstzweck übergeordnet[3]). Dieses von den geltenden

[2]Vgl. Rud. Richter: Schutz der Tier-Namen vor Emendation.—Senckenbergiana, **32**: 357-366, Frankfurt a. M., Februar 1952.

[3]Vgl. Rud. Richter: "Seiten-Priorität" ist keine Priorität.—Senckenbergiana, **33**: 73-74, Frankfurt a. M., Juni 1952.

Regeln abgelehnte und nun neu hervorgeholte "Prioritäts"-Prinzip soll sogar rückwirkend für alle Veröffentlichungen bis 1758 durchgeführt werden! Alle seitdem benützten Namen sollen geändert werden, wenn sich dabei erweist, dass sie der neuen "Seiten (usw).-Priorität" nicht entsprechen. Die Kommission erbietet sich zwar, "Namen von Wichtigkeit" auf besonders zu begründenden Antrag ausnahmsweise durch Aufhebung der Regeln zu retten; aber die Möglichkeit zu solchen umständlichen Einzel-Verfahren im Ausnahmefall fällt nicht ins Gewicht gegenüber der mutwilligen Änderung aller Namen im Normalfall.

4. Subjektive Homonymie soll einen Art-Namen für immer illegitim machen und zu einer Änderung des Namens zwingen. Nach den geltenden Regeln schläft ein Art-Name nur, solange er zu einer Gattung versetzt wird, in welcher derselbe Name schon mit einem früheren Datum vorhanden ist; aber er bleibt legitim und tritt unverändert wieder in sein Recht, sobald er der betreffenden Gattung nicht mehr zugerechnet wird[4]). Nach den Pariser Beschlüssen dagegen wird ein Art-Name für immer tot und daher ersatz-bedürftig, wenn er zu irgendeiner Zeit, vielleicht nur für ganz kurze Zeit und von einem Unkundigen (wenn nicht gar einem Tauf-Gierigen), in eine Gattungs-Kombination gebracht worden war, in der er vorübergehend ein subjektives Homonym gewesen ist. Für eine Flut von neuen Namen wird damit absichtlich ein Tor aufgetan[5]).

5. Nomina nuda und Manuskript-Namen sollen als legitimiert gelten, wenn sie in einer Synonymie-Liste erwähnt worden sind, und zwar auch dann, wenn der Autor der Liste einen solchen unfassbaren und daher gefährlichen Namen durch die Erwähnung unschädlich machen wollte. Er hat einen Stolperstein aus dem Weg geräumt, und die Literatur ist ihm dankbar gefolgt. Der gefährliche Name schien damit vorsorglich beseitigt zu sein. Bereits die geltenden Regeln bedürfen in diesem stabilisierenden Sinne eine noch klarere Fassunga ls bisher[6]). Stattdessen richten sich die Pariser Beschlüsse gegen die Stabilität: Jede Synonymierung soll als Legitimierung gelten! Die wohlüberlegte Absicht des Revisors soll in das Gegenteil verkehrt werden. Gewiss, für die Zukunft wird man sich dagegen schützen können, indem niemand

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[4]Vgl.: "Die endgültige und zeitweilige Verwerfung homonymer Art-Namen" in: Rud. Richter, Einführung in die Zoologische Nomenklatur durch Erläuterung der Internationalen Regeln," 2. umgearbeitete Auflage, S. 195-202, Frankfurt a. M. 1948.

[5]Obwohl die Pariser Beschlüsse noch keinerlei Rechtskraft besitzen, haben sie in der kurzen Zeit seit 1948 schon Schaden genug angerichtet. Ein Beispiel aus der Ornithologie, auf das man in Amerika aufmerksam gemacht hat: Ein Autor hat sich in Katalogen planmässig alle Arten zusammengesucht, die einmal durch vorübergehende Zurechnung zu einer bestimmten Gattung subjektive Homonyme geworden sind und veröffentlicht daraufhin Listen neuer Namen.

[6]E. Stresemann & E. Mayr: Über den Rechtsstand in Synonymielisten enthaltener Nomina Nuda.—Senckenbergiana, 32: 211-218, Frankfurt a. M. Juli 1951.

mehr Nomina nuda und Manuskript-Namen erwähnen wird. Aber für die Vergangenheit soll es keinen Schutz geben. Denn auch dieser neue Beschluss soll rückwirkend sein und allen als synonym erwähnten, mit Recht vergessenen Nomina nuda und Manuskript-Namen legitime Rechte verleihen auf Kosten der gebräuchlichen Namen.

C. Notwendigkeit einer Präambel für die Internationalen Regeln

Eine Präambel sollte den Internationalen Regeln als Richtschnur vorangestellt werden und folgende übergeordnete Sätze enthalten :

1. Der oberste Zweck der Regeln ist die Stabilität der Namen.
2. Wenn eine Bestimmung der Regeln mit dem Prinzip der Stabilität in Widerstreit steht, so ist es die Pflicht der Internationalen Kommission die Anderung der betreffenden Bestimmung herbeizuführen. Denn die Namen sind wichtiger als die Nomenklatur.
3. Die Zuständigkeit der Internationalen Kommissionen beschränkt sich auf die Nomenklatur und erstreckt sich nicht auf Fragen der Taxonomie.

D. Notwendige Beschleunigung auf dem Wege zu einer stabilen Nomenklatur

1. Die Regeln sollten zum Schutz gegen die Exhumierung vergessener Namen eine Verjährung der Priorität einführen. Nach Vorschlägen, die bereits in Nord-Amerika gemacht worden sind, etwa in folgender Weise : Ein Name verliert seine Priorität, wenn er während 50 Jahren in weniger als 5% derjenigen Veröffentlichungen benützt worden ist, die sich mit der betreffenden Einheit beschäftigt haben. (Formulierung von E. Mayr, New York.)

2. Zusammenfassende Werke sollten im Block mit allen darin enthaltenen Namen der Gattungen und deren Typus-Arten endgültig als Nomina conservata auf die Offizielle Liste gesetzt werden. Es gibt solche Werke, in denen anerkannte Spezialisten die Gattungen (mit den Genotypen) grösserer Gruppen zusammenfassend dargestellt haben. Ausschüsse von Spezialisten, berufen von den wissenschaftlichen Gesellschaften, könnten solche Werke der Internationalen Kommission empfehlen und den Schutz der darin enthaltenen Namen beantragen.

Als Beispiel sei der " Treatise on Evertebrate Paleontology " genannt, an dem Spezialisten fast aller Länder ihre Gruppen bearbeiten. Die Anerkennung des Treatise in diesem Sinne, wie sie bereits bei der Kommission beantragt worden ist[7]) würde ein märchenhaft erscheinendes Ziel mit einem Schlage verwirklichen : Praktisch sämtliche Gattungs-Namen (mit Genotypen) der fossilen Wirbellosen würden jeder künftigen Erörterung enthoben sein. Ein gewaltiges Kapital an Geld und noch mehr an Zeit wäre dadurch für wirkliche Forschungs-Aufgaben gewonnen. Ein solcher Präzedenz-Fall würde die Entwicklung der Zoologischen Nomenklatur auf einen neuen Weg und schnell zum Ziele führen.

[7]Rud. Richter : Antrag.—*Bull. zool. Nomenclature*, **6** : 184, London, May 1952.

Sollten sich dann wirklich für den einen oder anderen Namen begründete Bedenken ergeben, dann ist es die unvergleichlich geringere Mühe, diese einzelnen Fälle als Ausnahmen durch die Kommission mit Hilfe der Suspension zu lösen. Die Kommission würde also nicht sofort völlig arbeitslos werden.

Wenn sich dagegen auch in Zukunft die Kommission darauf beschränkt, jeden Namen einzeln in seiner Historie zu untersuchen, und zwar oft unwichtige und nur durch Zufall aufgetauchte Namen, und diese dann nach jahrelangen Sonder-Verhandlungen und in jedem Einzelfall mit grossem Aufwand an Druckwerk zu schützen, so werden noch Hunderte von Jahren vergehen, bis das Ziel erreicht ist, zu dem man einst hoffnungsvoll die Regeln und die Kommission geschaffen hat.

3. Eine kurze und klare Fassung aller Regeln ist erforderlich und wäre leicht möglich.

Für die geltenden Regeln, die noch den Geist von R. Blanchard und Franz Eilhard Schulze atmen, wäre der logische Einbau aller etwaigen neuen Bestimmungen einfach. Für die Neu-Fassung einiger Artikel sind bereits von verschiedenen Seiten Vorschläge veröffentlicht worden. Grundsätzlicher Änderungen bedarf es dabei nirgends; solche wären auch nicht im Interesse der Kontinuität und Stabilität.

Dieselbe Forderung wird erhoben für die " Gutachten " (opinions) und für alle Verhandlungen der Internationalen Kommission. Die Grundsätze und Ausführungs-Bestimmungen sind an sich logisch und einfach; sie können, wenn man es ernstlich will, in der schlichten Sprache des Lebens ausgedrückt werden. Leider ist die Kommission in den letzten Jahren zu einer umständlichen Juristen-Sprache übergegangen, welche die Klarheit nicht erhöht, aber die Verständlichkeit so erschwert, dass kaum ein Zoologe davon Notiz nimmt. Die ständigen Wiederholungen unnötiger Floskeln machen diese Veröffentlichungen dickleibig; der Preis wird so hoch, dass sie nur an sehr wenigen Stellen zugänglich sind.

Die wissenschaftliche Welt ist daher über die Vorgänge und Pläne auf dem Gebeit der Nomenklatur ohne Unterrichtung geblieben. Daher kann auch dieser Einspruch gegen die Pariser Beschlüsse erst heute, im letzten Augenblick vor dem auf den 1. Juli 1952 festgesetzten Termin, erfolgen.

Im Auftrag folgender Gesellschaften:

Deutsche Zoologische Gesellschaft:

E. Stresemann, K. Günther, O. Koehler, H.-J. Stammer

Paläontologische Gesellschaft:

Rud. Richter, R. Dehm, J. Görges, W. Gross, H. Hiltermann, O. Kühn, O. H. Schindewolf, Hermann Schmidt.

Senckenbergische Naturforschende Gesellschaft:

R. Mertens, E. Franz, Rud. Richter, W. Schäfer, Herta Schmidt, J. Steinbacher, W. Struve, E. Triebel, A. Zilch.

DOCUMENT 1/35

Views of MEMBERS OF THE SOCIETY OF SYSTEMATIC ZOOLOGY on the question of the Law of Priority

Letter, with enclosure, dated 19th June 1952, from Professor CHARLES D. MICHENER, former Chairman of the Nomenclature Committee of the above Society (letter transmitted by Dr. W. I. FOLLETT, Professor Michener's successor as Chairman of the foregoing Committee)

The Nomenclature Committee of the Society of Systematic Zoology has recently made a study of a number of nomenclatorial problems. In some instances it has sent a ballot to members of the Society in order that its opinion and recommendations to you would correctly reflect the viewpoint of American taxonomists.

The material on the enclosed sheets deals with the results of such ballots. It is transmitted to you with the request that the members of the International Commission on Zoological Nomenclature be notified of the stand of the Society on the problems concerned, and that, where rules or opinions are involved, this stand be considered in connection with the proposed revisions of the *Règles*.

Editorial Note : *The document here reproduced was the only one of the enclosures to Professor Michener's letter which was concerned with the problem of stability and the Law of Priority and is therefore the only one of those enclosures reproduced at this point* (*intl'd F.H. 10th April* 1953).

Enclosure to Professor Michener's letter

Law of Priority

It appears that the Law of Priority, which many taxonomists have regarded as the cornerstone of nomenclature, is now under strong attack in some quarters (cf. Hemming, 1950, Minutes of the Meeting of the ICZN, 1948, in *Bull. zool. Nomenclature*, vol. 4, p. 233. " No member of the Section spoke in favour of the continuance of the present system under which the Law of Priority not only failed to promote uniformity but was itself actively instrumental in leading to confusion and instability in nomenclature ").

In order to determine the views of American taxonomists on this matter, the ballot shown below was sent to members of the Society of Systematic Zoology.

1. I favour the STRICT application of priority, with no exceptions.

2. I favour REASONABLY STRICT application of priority, with few exceptions, that must be virtually universally approved (e.g., *Musca domestica*, *Plasmodium*, etc.), where confusion would result in other fields as well.

3. I favour MODERATE APPLICATION of priority, with more exceptions than in No. 2, but applications for these exceptions must be carefully and fully documented and must receive general approval. Generally, exceptions should be made only for cases of real confusion, i.e., not for straight synonymy but to prevent REVERSAL OF USAGE in important names.

4. I favour PRIORITY as a general principle, but with reasonably free exceptions, to be made in any group whenever the specialists in that group so agree, regardless of how important the case is and whether confusion or mere synonymy is involved.

5. I favour USAGE as the guiding principle, regardless of priority.

The voting on these choices was as follows :—

(1) 33 (Strict application of priority).

(2) 119 (Reasonably strict application of priority).

(3) 187 (Moderate application of priority).

(4) 29 (Free exceptions).

(5) 3 (Usage as the guiding principle).

In view of these returns, and also in view of the consensus of opinion among the members of the Nomenclature Committee of the Society of Systematic Zoology, it is recommended that the International Commission of Zoological Nomenclature consider setting aside the law of priority only when carefully and fully documented cases are presented to it, and then only (1) when virtually universally approved names (e.g., *Musca domestica*, *Plasmodium*) are to be conserved or (2) when real confusion such as reversal of usage would result from strict application of the law of priority, or (3) when changes in subfamily or family names would result from strict application of the law of priority.

CHARLES D. MICHENER,
Chairman, Nomenclature Committee,
Society of Systematic Zoology.

DOCUMENT 1/36

By ANGEL CABRERA
(*Eva Peron, F.C.N.G.R., Argentina*)

Statement dated 22nd June 1952

ON THE LAW OF PRESCRIPTION. Z.N.(S.)359

I *do not* consider that a Law of Prescription would be the best means of promoting the stabilisation of Zoological Nomenclature. On the contrary, such a law would seem to me the best way to go back to the chaotic situation which existed prior to the adoption of the *Règles*. Personally, I am somewhat sceptical about stability of Nomenclature. Every human thing is unstable, specially so laws and regulations voted in international congresses, in which by reasons of distance and travel expenses the influence of a local majority is exaggerated. Whether our efforts will end in relative stability remains to be seen, but common sense tells us that we can approach such an ideal only through objective, impersonal means, and not by subjective and changeable ways. The Law of Priority is not perfect, but it is at least a sound, objective

law, resting on the neutral and impersonal basis of the date of the beginning of binominal nomenclature. A Law of Prescription—apart from the weak points so aptly pointed out by yourself—would be a subjective law, based on an arbitrary date and therefore open to constant modification. I suppose the date 1850 in Dr. Lemche's proposal has been selected with a view to the difficulties about old zoological literature. Since no less than forty-four Danish zoologists advocate this proposal, it is possible that it may be approved in the Copenhagen Congress. Well, the next Congress, or the one following the next, may transfer the Prescription date to 1950, or to any other year, on the ground that the two World Wars involved a serious drawback for scientific research and scientific intercommunication. The principle of Prescription has not yet been discussed and we already have two or three different opinions about its regulation. That suffices to show that, in whichever way we see the matter, it is not by falling into the complications and discrepancies of Prescription that we can escape the disadvantages of Priority.

DOCUMENT 1/37

By ANGEL CABRERA
(*Eva Peron, F.C.N.G.R., Argentina*)

Statement dated 22nd June 1952

ON "NOMINA DUBIA." Z.N.(S.)359

The *nomina dubia* question is more taxonomical than nomenclatorial, but I think it absolutely necessary to have in the *Règles* some provisions about such names, this being one of the cases in which taxonomy and nomenclature are inseparable. The suggestions about the treatment to be given to generic *nomina dubia* of classes (1) and (2) are quite acceptable. As for class (3), I dare to suggest the inclusion in the *Règles* of a provision ruling that, when a nominal genus based on an unrecognisable species has been accepted by specialists during the fifty years following its publication to include species supposedly congeneric with the unrecognisable type, that nominal genus must be declared valid, and its new type should be the first recognisable species selected as such either by the author of the genus or by another author. As to trivial *nomina dubia*, I think that in cases where the specialists are utterly unable to identify the corresponding taxonomic species, it is necessary to distinguish two classes: (1) names of species unrecognisable because the identification is impossible without seeing the type material, and this type material is lost (e.g. *Simia apedia* Linnaeus, 1758, not determinable from description and based on a lost specimen); and (2) names established on a fantastic or imaginary basis (e.g. *Megophias megophias* Rafinesque, 1817, based on an imaginary sea serpent). In my opinion, names in class (1) must be rejected for the purposes of the Law of Priority, but not for those of the Law of Homonymy, whereas names in class (2) must be rejected for the purposes of both laws. In fact, these names, although sometimes accepted, fall under the scope of *Opinion* 2 and of the Recommendation agreed to in Paris (*Bull. zool. Nomencl.* **4** : 144), as they are not based on actual things.

DOCUMENT 1/38

By JOSHUA L. BAILY, Jr.
(*San Diego, California, U.S.A.*)

Statement received on 24th June 1952

REFERENCE Z.N.(S.)359 : *Bull. zool. Nomencl.* **7**: 174, Paragraph 23(4)

Examples of specific trivial names which are also the names of types localities are *Sonorella mustang* Pilsbry and Ferriss, from the Mustang mountains, and *Vespericola columbiana shasta* Berry, from Mt. Shasta.

Personally I disapprove of such names, and would like to have the Commission issue a ruling that when a geographic name becomes a trivial name it should first be Latinized (unless it already has the form of a Latin noun) and then either given the genitive form or an appropriate termination such as "*-ensis*" or an equivalent.

I would caution against adoption of a rule declaring such localities as type localities. Generally they are such, but not always. Sometimes such names are given as the result of a misunderstanding of the facts. *Pecten magellanicus* does not come from the Straits of Magellan but from New England. *Cassis madagascariensis* does not come from Madagascar but from the West Indies. *Unio peruvianus* does not come from Peru but from the Ohio River.

Whether these names should be changed by suspension of the rules is a question that will someday come before the Commission. Until that time it would seem to me wiser to defer the action contemplated by this application, desirable as it may be in most instances.

DOCUMENT 1/39

Statement furnished on behalf of the

SCIENTIFIC STAFF OF THE ROYAL ONTARIO MUSEUM OF ZOOLOGY AND PALAEONTOLOGY, TORONTO, CANADA

Letter, dated 26th June 1952, from F. A. URQUHART, Director

As you are no doubt aware, the Royal Ontario Museum of Zoology and Palaeontology is rapidly becoming the centre for the study of systematic zoology in Canada. In view of the fact that we now have a trained staff of systematic workers, I thought it advisable to bring the various matters contained in the publications of the International Commission on Zoological Nomenclature to their attention. After due consideration we held meetings to discuss the various problems upon which you have asked for advice.

I most humbly submit to you some suggestions and conclusions which we as a body of systematic workers have drawn up for your consideration.

I do sincerely hope that we may have added clarification, rather than confusion, to some of the problems which are now being considered by you for presentation at the next annual congress. I realise only too well the many difficulties that are involved. I also realise that it would be quite impossible to formulate any set of rules and expect to get a unanimous approval on all of them. It is, however, my sincere hope as Director of one of the largest systematic institutions in North America that we shall eventually arrive at simplified rules which can be grasped by the beginning student, and rules which when followed by all workers will eventually lead to extreme clarification.

May I take this opportunity to congratulate you on the very fine work which you have been doing. The manner in which you have outlined the various problems concerned and the amount of research which you have done on these various problems is most commendable. May I wish you every success in your very great and very important task.

Enclosure to Dr. F. A. Urquhart's letter of 26th June 1952

We unanimously agreed that the stabilization of zoological nomenclature could best be secured by means of amplifications, clarifications and extensions carried out within the existing framework of the rules and by the more extended use of the existing provisions for stabilizing individual names through the *Official Lists*.

We felt that it would be helpful if a preamble introduced the entire code, making clear the purposes for which rules of zoological nomenclature have been established.

We agreed with the suggestion that, as part of any general plan for the promotion of stability in nomenclature, the plenary powers provision should be reworded by the omission of the phrase " greater confusion than uniformity " and having inserted in its place a condition that the plenary powers are for use where, in the judgment of the Commission, the use of those powers is clearly necessary for the purpose of promoting stability and uniformity in zoological nomenclature.

We agree with the suggestion submitted by Dr. Mayr and outlined by you in paragraph 15, page 164 of volume 7, parts 5 and 6.

We unanimously agreed with the suggestion set forth by you in paragraph 16, page 167 of volume 7, parts 5 and 6.

We unanimously agreed with the suggestion set forth by you in paragraph 18, pages 169-170. We again suggest however, that application to the Commission on Zoological Nomenclature might be made through the person or persons in charge of the Systematic Studies for a particular Order who, as previously outlined, would be in communication with other workers in this particular field.

We unanimously agreed with the provision as set forth by you in paragraph 23, page 174 of volume 7, parts 5 and 6.

DOCUMENT 1/40

By A. MYRA KEEN and SIEMON S. MULLER
(*Stanford University, Stanford, California, U.S.A.*)

Enclosure to a letter dated 1st July 1952

COMMENTS ON THE QUESTION OF MEANS FOR PROMOTING THE GREATEST POSSIBLE STABILITY IN ZOOLOGICAL NOMENCLATURE

The " Law of Prescription " seems to us to be, at this late date, not practicable. Had it been promulgated some fifty years ago, before many long-forgotten works had been exhumed, it might have been useful. Its application now would only lead to confusion.

As to means of promoting greater stability, we feel that strict adherence to the *Règles* as now in effect, or at least to the *Règles* as they will be when the clarifications adopted by the 1948 Congress are included, will lead to stability. We deplore too frequent resort to the use of the plenary powers of the Commission, for the result of such action will be to set up an esoteric body of knowledge available only to those who have access to the facilities of a large library or who can afford to subscribe to the publications of the Commission and also to those persons having leisure and inclination to read a large volume of output. Priority may at times require the dropping of a familiar name and the learning of an unfamiliar one ; but this, we feel, is much to be preferred to the uncertainty that results when suddenly one discovers that by action of the Commission, priority has been set aside to favour the wishes or convenience of some petitioner. As one of our correspondents has wryly said of such action, " This cuts the ground from under us."

DOCUMENT 1/41

Statement furnished on behalf of the NOMENCLATURE COMMITTEE AT THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK, U.S.A.

Extract from a letter dated 10th July 1952, from JOHN T. ZIMMER

As I wrote you before, I have had considerable difficulty in attempting to get the Committee's action on the various proposals on which you wished our opinion. As I said then, it is a bad time of year, since many of the members of the staff and of the Committee are away for the summer, and some left before the matter could be brought to their attention. Consequently, I have no full Committee action to report. However, I have drafted our reply and suggestions and have added the names of the individuals who approved them as written ; some of them are not members of the Committee.

Enclosure to Dr. John T. Zimmer's letter of 10th July 1952

MEANS FOR SECURING STABILITY IN NOMENCLATURE Z.N.(S.)359

Under this heading is the question of a " Law of Prescription " as against removal of obscurities in existing rules. The former denies priority to names that have been unused for 100 years following original publication, but its adoption and use would entail exhaustive searches through all literature, probably an impossible task. Dr. Mayr suggests 50 years and excludes mere citations in synonymy from consideration as " usage." It might be possible to require evidence of consistent usage in even moderate degree in comprehensive treatments to establish usage, not complete absence of such ; a modified " auctorium plurimorum " plan with a " Statute of Limitations." Such a statute presents many difficulties of adequate formulation and should be thoroughly discussed before final adoption. It is possible that a 95 per cent. usage other than in synonymies, bibliographies, and compendiums such as Sherborn's " Index Animalium," should constitute adequate acceptance.

The removal of obscurities in existing rules should also be accomplished, but not necessarily as an alternative to the Law of Prescription, if adopted. It should include a " Sub-Title or Preamble " defining the primary purpose of the Code as to ensure stability of nomenclature.

An *Official Index* of rejected and invalid books should be useful.

Elimination of generic and specific names of indeterminate application. This should aid in stability. The names will be rejected for the purposes of priority but not of homonymy.

Elaborate rules have been proposed for fixing type localities : original designation, monotypy, restriction to originally included localities in subsequent designation, etc. Correction of erroneous original localities to be by the Commission. In subsequent fixation, if supposedly lost holotype is rediscovered

and has a different locality from that selected, the case is to be referred to the Commission. Similarly, if overlooked data are discovered in books or manuscripts by the original author upsetting the designated locality, the case will go to the Commission. Similar rules are proposed for horizons of fossil species ; also for hosts of parasitic species. On the whole, such rules ought to be fairly simple to operate, although it is questionable how much should be put on the shoulders of the Commission. Common sense should dictate the selection of type localities from among those from which the type could have come, and on discovery of error could be automatically corrected by the first reviser principle. No elaborate rules are required. The Commission should not be saddled with more work than it can accomplish without long delays.

A.M.N.H. Committee on Nomenclature

(Signed) Edwin H. Colbert
John T. Nichols
Ernst Mayr
George H. H. Tate
John T. Zimmer (Chairman).

DOCUMENT 1/42

Statement furnished on behalf of the

NOMENCLATURE DISCUSSION GROUP OF WASHINGTON, D.C., U.S.A.

Enclosure to a letter, dated 16th July 1952, from R. E. BLACKWELDER, Secretary

RESUBMISSION OF PETITION REGARDING THE PLENARY POWERS* Z.N.(S.)359

The Nomenclature Discussion Group of Washington, D.C., under its then used name of Smithsonian Institution Committee on Nomenclature, submitted to the Secretary of the International Commission on Zoological Nomenclature prior to the Paris Congress a petition on the subject of the use of the Plenary Powers to establish *nomina conservanda* (*Science* **107** : 543–544, May 21, 1948). This petition was supported by votes and comments from a number of American zoologists. The letter of transmittal submitted this material for presentation and discussion at the Paris Meeting of the Commission.

Unfortunately this petition and the supporting material were not presented at Paris. On the other hand, Dr. Henning Lemche did present a petition on a different viewpoint, on behalf of a large number of Scandinavian zoologists. Furthermore, during the Congress there was received, and at once presented, a letter from Austrian zoologists stating views similar to those of Dr. Lemche. Both of these have been published in full with all signatories in the reports of the nomenclature proceedings of the Paris Congress (*Bull. zool. Nomcl.* **3** : 158-161 and **5** : 77-78, 1950). In the light of the foregoing, the omission of our petition from both presentation at the meetings and the printed minutes of the meetings has resulted in a rather onesided appearance of the views of zoologists, an appearance which has been referred to with some emphasis (Hemming, 1951, *Trans. Soc. Brit. Ent.* **2** : 1-15 ; Hemming, 1952, *Bull. zool. Nomencl.* **7** : 148-188).

In view of the recent appeal for statements on the general problem of stability in nomenclature, we have examined again our petition, and find that it still outlines the position that we wish to present for the consideration of the International Commission on Zoological Nomenclature. Accordingly we hereby formally resubmit the petition for the Nomenclature Discussion Group. The petition is already in the files of the Commission, but two copies of the printed petition are appended.

We have not republished this petition, nor contacted the previous supporting signers, and accordingly we do not formally resubmit the supporting material at this time. However, we do wish to call attention to the fact that of the 170 zoologists who voluntarily responded on the original petition, 156 supported our petition, and 7 would even have gone much farther in limiting the plenary powers. It is doubtful that there would be much change in the votes at the present time.

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*See immediately following Editorial Note.

Editorial Note

The document enclosed with the foregoing letter was the same as that which had been enclosed with Dr. Curtis W. Sabrosky's letter of 25th June 1948. In view of the fact that that document had been earmarked for consideration at Copenhagen when the discussion on the proposed introduction of a Law of Prescription came to be resumed from the point at which it was adjourned at Paris in July 1948 (1950, *Bull. zool. Nomencl.* **4** : 233), I reached the conclusion, when I came to prepare the present volume for publication, that the most appropriate course would be to publish the document in question, together with Dr. Sabrosky's letter of June 1948, at the appropriate point in the historical series in which it had been decided that communications received on the subject of the stabilisation of zoological nomenclature should be arranged, and to publish at the point appropriate for documents dated July 1952 the covering note enclosed with Dr. Richard E. Blackwelder's letter of 16th July 1952, while not reprinting the longer paper submitted in June 1948, since under the procedure proposed that document would already have been published in the present volume in its historically correct place, namely as the enclosure to Dr. Sabrosky's letter of 25th June 1948.

2. Accordingly, in a letter dated 17th January 1953 I put the foregoing suggestion to Dr. Blackwelder, by whom the document referred to above had been resubmitted to the Commission and who had succeeded to the position formerly held by Dr. Sabrosky as Secretary of the body which had in the mean time been renamed the Nomenclature Discussion Group, Washington, D.C. As will be seen from the extract from Dr. Blackwelder's reply of 5th February 1953, given in the annexe to the present note, he concurred in the procedure which I had suggested.

3. Accordingly, the statement prepared by the Smithsonian Institution Committee on Nomenclature, transmitted under cover of Dr. Sabrosky's letter of 25th June 1948, is reproduced as Document 1/8 in the present series (see pp. 9-11), while the covering note prepared by the Nomenclature Discussion Group, when resubmitting the foregoing statement (under cover of a letter, dated 16th July 1952, by Dr. Blackwelder) is reproduced here as Document 1/42, but without the document then resubmitted, that document appearing (as explained above) in its place as the annexe to Dr. Sabrosky's letter of 25th June 1948.

(intl'd) F. H., 25th February 1953.

Annexe to Editorial Note

Extract from a letter dated 5th February 1953, from Dr. Richard E. Blackwelder, Secretary Nomenclature Discussion Group, Washington, D.C.

Perhaps our " resubmission " of the application on the use of the plenary powers, and the wording used in referring to the original documents was not as clear a statement of our intention as we should have made. The original (1948) application was submitted for consideration by the International Commission on Zoological Nomenclature and is presumably still before them.

The resubmission was intended merely to call attention to it and reiterate our belief in the contents. Our inability to speak again for the original supporters in no way affects the original proposal or their support of it.

It appears to us that the original application is the one to be considered, with our resubmission as possible supplementary matter. No doubt it would also be useful to include the pertinent paragraphs from Mr. Sabrosky's letter of June 25 1948. because they constitute a summary of the attached papers.

DOCUMENT 1/43

By C. W. WRIGHT
(*London*)

Enclosure to a letter dated 18th July 1952

ANSWERS TO QUESTIONS RAISED IN PARAGRAPH 39 OF THE PAPER BY THE SECRETARY TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE (1952, *Bull. zool. Nomencl.* **7** : 187-188)

Question (1) A Law of Prescription would be a confession of defeat. I see no reason, taking a long view, for pessimism. The Rules must be amended if necessary to deal with every type of problem that crops up. Given common sense and the Rules difficulties will be resolved. The great peril to nomenclature lies in the fact that many authors are both ignorant and selfish. Several prominent palaeontological monographers of my acquaintance either do not trouble to apply the Rules or if they do try to apply them they misunderstand them.

(3) The main solution to the difficulty of ancient works is for authors to get into the habit of considering the possibility and advisability of applying for such works to be placed on the *Index* before starting to use names excavated from them.

DOCUMENT 1/44

By the NOMENCLATURE COMMITTEE AT THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK, U.S.A.

Letter, dated 18th July 1952, from ERNST MAYR

I am sending you herewith an additional petition to the Commission concerning a Preamble to the *Règles*.

Enclosure to Dr. Ernst Mayr's letter of 18th July 1952

It has been criticised by numerous taxonomists that the present version of the *Règles* does not contain an adequate statement of the object of the rules of nomenclature. To meet this criticism we propose that the following preamble of the Rules be adopted.

PREAMBLE

The object of the Rules of the Zoological Nomenclature is to establish the basis for a uniform and stable set of zoological names. All individual provisions of the Rules are subservient to this ultimate goal. If the application of the Rules in an individual case threatens the stability of nomenclature such a case shall be brought to the attention of the International Commission for possible exercise of its Plenary Powers. If the application of a given Rule consistently disturbs the stability of nomenclature, it shall be the duty of the Commission to modify said rule in such a manner as to restore stability.

Signed :
The Nomenclature Committee at the American Museum of Natural History, New York.
Ernst Mayr
John T. Zimmer
G. H. H. Tate
C. H. Curran

DOCUMENT 1/45

By ERNST MAYR (*The American Museum of Natural History, New York*)

Enclosure to a letter dated 18th July 1952

The Commission is herewith petitioned to redraft Article 25. The provisions now included in Article 25 should be separated into two articles, one dealing with the availability of names and one with the validity of names. In the former article all provisions concerning proper publication of names should be included.

The article on validity of names should contain provisions concerning synonymy and homonymy, as well as a statute of limitations. It should

include a statement on priority as well as rules specifying under which conditions priority may be set aside. A suggested draft of such specifications is attached.

Enclosure to Dr. Ernst Mayr's application of 18th July 1952

RULE OF VALIDITY

If two names are available for the same taxonomic unit, the earlier published available name ("senior synonym") is the valid name unless it is invalid owing to homonymy or unless the later published name ("junior synonym") has special rights as *nomen conservatum* or *nomen conservandum.*

A *nomen conservatum* is a name that has been preserved by a special decision under the plenary powers of the International Commission and which has been placed on the appropriate "List of Official Names."

A *nomen conservandum* is any name belonging to either of the following categories of names:

(1) Any name on one of the *Official Lists of Names in Zoology* (excluding lists of names not available) but concerning which the Commission has not taken action under its plenary powers.

(2) Any name in current usage for the conservation of which a petition to the Commission is pending.

A *presumptive nomen conservandum* is any name belonging to either of the following categories:

(1) The name of any genus that is the type-genus of a super-generic category.

(2) Any name (excluding a binomial or trinomial combination) that has been applied exclusively or virtually exclusively to a taxonomic unit for a period of fifty years or more. Such a name must have been used in at least ten separate publications in order to qualify. This period may be shortened to not less than 30 years for names that have been used in 100 publications or more during that period.

The law of priority does not apply to *nomina conservata.* It shall not be applied to *nomina conservanda* or presumptive *nomina conservanda* in such a manner as to disturb current usage, or the user's view as to what is current usage, unless the Commission so directs. When a taxonomist discovers that the application of priority to a presumptive *nomen conservandum* would disturb usage, it shall be his duty to present the case to the International Commission, but to conserve current usage pending their decision.

When a presumptive *nomen conservandum* is thus laid before the International Commission, that body shall give it due consideration and shall determine whether the name in question shall become a *nomen conservatum* or whether the *Règles* should, in this case, take their normal course.

DOCUMENT 1/46

By HENNING LEMCHE

(*Universitetets Zoologiska Museum, Copenhagen, Denmark*)

Letter dated 20th July 1952

(See also Document 1/7)

To the main question about stabilizing nomenclature, viz. the proposed Rule of Prescription (we have not and cannot have real laws), I would like to express my regret that you seem to have misunderstood the way in which it is intended to act. Especially, I felt a little disappointed when I read your suggestive heading to your par. 7 : " The weakness of any Law of Prescription arising from the negative character of the test imposed." There is no weakness here, as the whole proposal is positive.

The thing to do is to rule that every case where an old name is found and an author wishes to use it, he will have to cite *both* the old quotation and at least one newer record indicating that the name has been used more recently. If he cannot find any newer record, he has no right *to alter* current usage. So, contrary to the rule of priority, the burden of looking through literature is entirely on the man who wishes to overthrow current usage. The weakness of the priority rule has always been the impossibility to state that a name is really *the* first one. Therefore, your main objections to my proposal could be applied with their full force against the rule of priority, but not against a rule of prescription where the proof that the name is available lies in the presentation of a later quotation (from a place where it is used, *not* in any synonymy).

The proposed rule is exclusively for future use, so that no new disturbance can be started. Moreover, no library difficulties can be involved, because if the author has the book and finds the old name, he must *not* use it, he will have to keep to *current usage*, and let the man near a great library have the sole chance to disturb nomenclature. If, in future, an author revives an old name, other authors should not follow him if he gives no newer citation, because the procedure is incorrect and the change invalid.

To your alternative proposals, I totally agree that it is very good to make the *Règles* as definite and sharp as possible. But, again, we come to our main difficulty : the difference between your deductively trained brain and the inductively trained ones of the zoologists who are to use your text.

I prefer, a *single*, definite article about the plenary powers, but no other references thereto. If we add such references to some articles, there might be authors believing therefrom in that the plenary powers can not be used concerning any other articles. So, we will fall in the pit we are trying to escape.

The *Official Index of Rejected and Invalid Books* seems a good idea. An asterisk could indicate volumes, the names of which would be " sympathetically regarded " by the Commission for use of the plenary powers.

DOCUMENT 1/47

By JOHN H. LOCHHEAD

(*Department of Zoology, University of Vermont, Burlington, Vermont, U.S.A., and Marine Biological Laboratory, Woods Hole, Massachusetts, U.S.A.*)

Enclosure to a letter dated 21st July 1952

COMMENTS ON THE MEANS TO BE FOUND FOR PROMOTING THE GREATEST POSSIBLE STABILITY IN ZOOLOGICAL NOMENCLATURE

Since the comments on the *Limulus* versus *Xiphosura* controversy which I am enclosing herewith have some bearing on the general question of how best to promote uniformity and stability of taxonomic names, you may wish a separate statement from me on this latter topic.

Some general conclusions that seem to me to emerge from the *Limulus* versus *Xiphosura* case are the following. 1. The *Official Lists* must be published and made easily accessible to all zoologists. 2. The legal force of the *Official Lists* must be made clear and widely known among zoologists. 3. In particular, a name once on an *Official List* should not be removed because of new discoveries in the literature, except when the Commission may decide that such removal really would be in the best interests of taxonomic stability and uniformity. 4. The *Official Indexes of Rejected and Invalid Names* also should be published and made easily accessible to all zoologists. 5. An " Official Index of Rejected and Invalid Books " (rather than " Books " I would say " Publications ") should likewise be published and made widely accessible. 6. There should be full publication of the arguments involved in each case considered by the Commission.

All of the above proposals have, of course, already been acted on by the Commission or are set forth in Secretary Hemming's report. It is my belief that had they all been in force some years ago, we would not have had such disputes as that involved in the case of *Limulus* versus *Xiphosura*.

It will be seen that I favour developments carried out within the existing framework of the *Règles* rather than a Law of Prescription. As pointed out by Secretary Hemming in paragraph 7, the latter proposal has the same weakness as does unyielding adherence to the Law of Priority—namely that it demands a knowledge of the entire literature before the stability of a name can be assured.

In reference to discoveries in the literature which under the rules would require the changing of long established taxonomic names, it seems to be assumed (in paragraphs 34-36 of Secretary Hemming's report) that the specialists who make such discoveries will voluntarily apply to the Commission for validation on the *Official Lists* of the established names, and rejection on the *Official Indexes* of the newly discovered competitors. Perhaps I have overlooked a proposal already made, but at any rate I would like to suggest that some sort

*The reference here is to Case Z.N.(S.)506, for which see 1951, *Bull. zool. Nomencl.* **2**:319-322.

of obligation be placed on zoologists to consult the Commission before displacing a long established name on a merely legal technicality. The main objection might be that publication of taxonomic papers would be held up while awaiting a decision by the Commission. However, it would seem to me that pending such a decision a genus or species could be referred to by its established name, with a note to the effect that the name is one currently under consideration by the International Commission. As a non-taxonomist, I cannot help feeling that perhaps some taxonomists take a wicked delight in changing names, a pleasure that should be denied them so far as possible under the *Règles*.

In regard to the proposal in paragraph 15, of an " Official Index of Rejected and Invalid Books " [or " Publications " ?], might there not also be use for an " Index of Publications partly or wholly approved by the Commission "— with references to the discussions and *Opinions* regarding them ?

DOCUMENT 1/48

By E. RAYMOND HALL

(*Department of Zoology, University of Kansas, Lawrence, Kansas, U.S.A.*)

Enclosure to a letter dated 22nd July 1952

Avoid extending list of *nomina conservanda* and employ instead the rule of priority with 1758 as the controlling date.

DOCUMENT 1/49

Statement furnished covering the views of three members of the NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY, U.S.A.

Letter, dated 27th July 1952, from W. I. FOLLETT, Chairman

STABILITY IN ZOOLOGICAL NOMENCLATURE

I enclose herewith comments on this subject, received from members of the Nomenclature Committee of the Society of Systematic Zoology.

Annexe 1 to the Chairman's letter of 27th July 1952

Extract from a letter, dated 20th May 1952, from HENRY TOWNES, *North Carolina State College of Agriculture and Engineering of the University of North Carolina, Raleigh, North Carolina, U.S.A.*

(7) I would suggest abolishing all exceptions to the rule of priority as the first step.

Annexe 2 to the Chairman's letter of 27th July 1952

Statement by JOHN T. ZIMMER
(*American Museum of Natural History, New York*)

359. Most of the proposals in this section are incorporated in previous sections.

A " Law of Prescription " might be highly desirable if it can be formulated in a workable condition. I do not believe it possible to insist on a name having been " unused " for 100 years or even 50 years following original publication, even excluding citations in synonymy from the category of usage. It is always possible for an obscure and utterly overlooked paper to be found that would cancel the non-usage provision. Possibly some sort of " auctorum plurimorum " principle could be devised that would recognise some general acceptance of a name for lengthy periods even if an occasional author adopted another available name. If the Commission were not so overworked, the *Official List* would offer an acceptable solution for this problem of stability.

I believe a " Sub-Title " of " Preamble " should be printed with the new Code, defining the primary purpose of the Code as the establishment of stability in nomenclature.

The proposed *Official Index of Rejected and Invalid Books* is desirable.

Elimination of generic and specific names of indeterminate application should promote stability. The names should be rejected for purposes of priority but not of homonymy.

The proposed rules covering fixation of type localities are too elaborate. They should be on a first reviser principle, with the requirement that the locality selected must be reasonable—a place from which the type could have come at the time it was presumably collected, a place on the itinerary of the known collector, a place from which the original author is known to have received material, certainly a place in the known range of the form, etc. Such designation is to be subject to revision if overlooked data are discovered that permit greater accuracy. Any disputed data may go to the Commission for action, but such cases should be few if the evidence is conclusive.*

Extract from a letter dated 21st July 1952, from Francis Hemming, Secretary to the International Commission on Zoological Nomenclature, to Dr. John T. Zimmer

Type localities ; I discussed this matter with Dr. Erwin Stresemann, when I met him in Lappland two years ago, and he then told me that in birds this did not give rise to difficulty. That in some cases it does is shown however by what has recently happened in the case of the name of the Syrian Ostrich, where, as there is at present no provision in the Code, Colonel Meinertzhagen has thought it necessary to make a special application to the Commission (shortly to be published in the *Bulletin*)[1], which would have been entirely unnecessary if the Code had contained rules on this subject. Speaking as a lepidopterist and not as a member of the Commission, I am quite certain that the lack of rules relating to this subject is one of the principal remaining obstacles to stability in nomenclature. The European fauna contains a much larger number of cases where species possess no type localities than any other, owing naturally to the fact that Linnaeus and the other early authors dealt with a relatively much larger number of European species than they did from any other part of the world—probably more than all other parts of the world put together. This problem is therefore one of special urgency and importance to European zoologists. It is clearly, however, not confined either to European workers or to the Lepidoptera, as may be seen, for example, in the arguments raised by the recent attempt by two American herpetologists to catalogue the type localities of the Reptiles and Amphibia of Mexico[2].

* See also in this connection, Document 1/42.

[1]See Meinertzhagen, 1953, *Bull. zool. Nomencl.* 9 : 96-97.

[2]See Smith (Hobart M.) & Taylor (Edward H.), 1950, *Sci. Bull. Univ. Kansas* 33 (Pt. 2) (No. 8) : 313-380.

Extract from a letter, dated 6th August 1952, from John T. Zimmer

I have delayed in replying to your letters of July 21. . . The difficulty has been that members of other Departments of the Museum are away on vacations and field trips. . . .

As regard type localities, we admit the desirability of fixing type localities. In my own case, I have proposed many such fixations or finer restrictions. The rules, however, should be simple and not necessarily rigid, if the reviser uses good judgment and selects a locality where the collector (if known) is known to have worked ; or a locality from which material was available to taxonomists in a possibly early time when the creature was originally described; or a locality not antagonistic to any data given by the describer ; or a locality from which material is now available showing agreement with the existing holotype, etc. However, future evidence may show the basis to have been faulty, in which case it should be remediable. Many times this will result in no confusion, although sometimes it may do so. Evidence for alteration would need to be positive and proof would have to be supplied by the would-be reviser.

As a case of what I mean, I may mention one of the humming birds discussed in one of my papers. Originally described from the " Upper Amazon " with certain characters given for it that are definitive. A more recent taxonomist proposed " Bolivia " as restricted type locality. I objected on the ground that Bolivia is not the "Upper Amazon" and Bolivian specimens lack the characters mentioned in the original description, which examples from localities on the Upper Amazon show well. Accordingly, I proposed a new restriction to an Upper-Amazonian locality, in all probability the exact place visited by the supposed collector of the original specimens. To necessitate holding to the first proposal of " Bolivia " would be in disregard of the weight of evidence, and yet it would have been difficult to outlaw such suggestion in advance. Our suggestion to require common sense in the selection of type localities by revisers would have done so much as elaborate rules. In this case two subspecies were involved.

Another case I have in mind. Count Berlepsch proposed a restriction of type locality for one of Spix's Amazonian birds to a certain locality. Hellmayr rejected Berlepsch's proposal on the grounds that Spix had never visited this place. I happened to come across a brief note in Spix to the effect that he had spent a night there. Consequently, I reverted to Berlepsch, but in this interchange there was not the slightest effect on the identity or validity of Spix's species. The same form occurs throughout the area in question.

In any case, we believe any rules for fixation of type localities should be simple, though authority for such fixation may well become a part of the new Code.

Annexe 3 to the Chairman's letter of 27th July 1952

Statement by W. I. FOLLETT

(*California Academy of Sciences, San Francisco, California, U.S.A.*)

With considerable diffidence, I venture to suggest the futility of our attempting to eliminate minor causes of instability in zoological nomenclature, while we continue to wink at the major cause.

As long as our rules continue to saddle nomenclature with the burden of expressing purely subjective notions of generic relationship, there can be no stability in nomenclature.

In order to avoid transitional inconveniences, we adhere to the system of the past 200 years, in disregard of stability and the requirements of the next 200—and 2000—years.

Supplementary statement by Dr. Follett in response to a question raised by the Secretary to the International Commission

Letter dated 18th February 1953

Your letter of 4th February 1953, concerning my comments submitted under date of July 27th 1952, has just arrived. With the exceptions hereinafter noted, I would answer your queries in the affirmative.

It was not my intention to express disapproval of measures to promote stability, such as those considered in volume 7, parts 5/6, of the *Bulletin of Zoological Nomenclature*. Instead, I wished to emphasise the apparent indifference of zoologists to the major cause of nomenclatorial instability, and to express concern that the frequent sharp protests should so often be directed against minor factors such as the law of priority rather than against that major cause.

I am convinced that the number of changes in nomenclature that have resulted from purely nomenclatorial factors, including priority, homonymy, emendations, position-precedence, and all changes that have been effected in the International Rules, comprise but an insignificant total in comparison with the number of changes in nomenclature that have resulted solely from shifts in generic evaluation. These shifts are based on subjective considerations, and are accordingly subject to continual modification.

In other words, the binominal system of Linnaeus itself appears to be the major cause of instability in nomenclature.

Theoretically, a uninominal system would therefore seem capable of eliminating a substantial element of instability, even though it would not eliminate variations inherent in the definition of species.

It appears unlikely that a uninominal system will be acceptable to zoologists, because of the transitional inconveniences that would arise from so drastic a change, and to a greater degree because of the prevalent desire that nomenclature express relationship.

I am unable, however, to avoid the conclusion that nomenclatorial expression of relationship is in irreconcilable conflict with the attainment of nomenclatorial stability.

DOCUMENT 1/50

By J. CHESTER BRADLEY
(*Cornell University, Ithaca, N.Y., U.S.A.*)

Statement dated 29th July 1952

(**Note :** The numbers cited in this statement refer to the paragraphs in the paper published by the Secretary to the International Commission on Zoological Nomenclature, 1952, *Bull. zool. Nomencl.* **7** : 148–188).

PROMOTING STABILITY IN NOMENCLATURE

A. Answers to specific questions in Section 39

(1) and (2). I consider that the stabilisation of zoological nomenclature would be importantly promoted by the introduction into the *Règles* of a *principle* of prescription. I do not say a law, because I do not see how it can be made so objective as to be automatically operative. Nevertheless, I think that great good would derive under the following provisions. (This suggestion is essentially and in brief a plan suggested by Dr. E. Mayr.)

(*a*) That any generic or trivial name that has been virtually exclusively in use for a given taxonomic unit for at least a specified number of years and has been used in at least a specified minimum number of publications shall be presumed to be a *nomen conservandum*.*

(*b*) That when any taxonomist observes that what he deems to be such a name is not tenable under some provision of the *Règles* he shall not disturb usage, but shall present the case to the Commission.

(*c*) That the Commission acting not under plenary powers, but by way of putting into operation the principle of prescription, in precedence over other provisions of the *Règles*, shall place the name upon the appropriate *Official List* as one to be conserved, provided in their judgement it qualifies, and such action is not for some reason contra-indicated.

By the preceding plan the subjective element expressed by the word " virtually " is not interpreted by the perhaps conflicting views of different taxonomists, but by central authority instead, and that is just where the existence of central authority provides efficiency.

3. Prescription is not an alternative to developments within the present framework of the *Règles*. Everything must be carried out that will contribute to stability.

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*Note that *nomen conservandum* means a name that ought to be conserved, as contrasted with *nomen conservatum* meaning a name that has been preserved, i.e., by official act of the Commission. This distinction of definition has been pointed out *in litt.* by Dr. E. Mayr and has been employed by Stresemann, Richter, Mertens and others (1952, *Senckenbergiana* **33** : 193-196). [J. C. B.]

4. *Particular suggestion for promoting stability.*

A regulation to be inserted in the *Règles* to the effect that: Whenever the status of a name having been correctly determined under the *Règles* and *Opinions* in force at the time, it has come into use as so determined, but some amendment to the *Règles* made subsequent to 1907 would enforce a change, it shall be presumed to be a *nomen conservandum.* It shall be the duty of every taxonomist becoming aware of such a name to present the facts to the Commission, who shall, without having need to apply their plenary power, decide the course to be pursued in the interests of continuity and stability. It is thus to be understood that no amendment made since 1907 is designed to overturn continuity or prevent stability, and *is not operative* in any case where in the opinion of the Commission it would do so.

B. Comments on numbered section of the Secretary's Paper

11. *Preamble.* A preamble is desirable.

I have seen the preamble suggested by the Committee on Nomenclature of the American Museum of Natural History. I like its brevity, and the way that it touches significantly upon just the right points. Its last sentence seems to me not to go far enough.

Before I read their suggestion I had written out some of my own, a copy of which I attach. Mine is far too long, nevertheless I think that some thoughts in it, especially clarification of the fields of nomenclature versus taxonomy are important and ought to be included.

In the Secretary's paragraph 11, I suggest that in the 7th line from the bottom, the word "objective" should be inserted before "species," and that the word "trivial" should be inserted before the word "name." (Let no impression get abroad that it is an aim to freeze specific names, i.e. generic and trivial combinations.) I should add at the end of the section that the system refrains from trespass upon the field of taxonomy. With these changes I fully endorse section 11.

12. *Plenary powers.* I strongly endorse this section. I should reword the last three lines to read "powers are for use where, in the judgment of the Commission, they will promote stability, continuity and uniformity in zoological nomenclature." As previously worded the last sentence places emphasis on whether it is clearly necessary to use the plenary powers to secure the objectives named, or whether they might be secured in some other way. My modification is to place emphasis on the objectives themselves as the important factor. I have added the word "continuity" because stability alone might be thought to mean stability from the time of action, without necessarily including continuity with the past.

13. *Obscurities and lacunae in the Règles.* Agreed.

14. *Stability despite emendation.* I strongly support this section. Instead of lengthening the *Règles* by frequent provisions of the sort indicated, each in connection with some particular instance, would not the drafting be greatly strengthened by a single general provision, somewhat along the lines suggested above under Section A (4).

15. *Official Index of Rejected and Invalid Books in Zoology.* I support the proposal for action. Since we do not wish to give the public impression that we are casting any stigma upon these books, or rejecting *en masse* as unreliable the biological or anatomical detail that they contain, would not the title better be " Official Index of Books Rejected or Invalid for the Purpose of Zoological Nomenclature " ?

In rejecting a work we reject every zoological name in it, unless special exception are made. Does this not imply that each such name should be entered in the appropriate *Official List of Rejected Names* ?

17. *Suppression of nomina dubia at the specific level which no specialist claims to recognise.* The fact that no specialist claims to recognise a name does not mean that no specialist can recognise it. No one may ever have had occasion to make a serious attempt to study the type. That is continually found to be the case, *ex. gr.* with North American insects the types of which are housed in Europe. We are confronted not only with what the specialist of the past or today has or can do, but what the specialist of tomorrow may do when he has opportunity. Instead of the provision in Article 31 (B.Z.N.**4** : 76 g.2) would not provision along the following line better serve such a case :

If the name in question has consistently had the status of a *nomen dubium*, and no type specimen is known to exist, the Commission may, upon request, suppress the name under their plenary powers.

If a type specimen is known to exist and the Commission is satisfied that adequate study has been given to the type, and that all are agreed that identification is hopeless, the Commission may, under their plenary powers either (a) suppress the name, or (*b*) cancel the type, thus paving the way to selection of a neotype, according to which may be the better course in the individual case, from the standpoint of continuity of nomenclatorial usage.

If the name has been traditionally employed to represent a certain taxonomic species, but due to the non-existence of a type, or failure to study the type, or unrecognisable condition of the type, doubt is now cast upon its actual identity and it is threatened with becoming a *nomen dubium*, the Commission may if necessary, cancel the type. The way will then be clear for establishing a neotype which will reflect the traditional sense in which the name has been employed.

18. *Nomina dubia which specialists cannot identify.* Of the alternative actions suggested for the Commission, I agree to (1) suppression of the name. I cannot agree to (2) in the form stated because that is a taxonomic question. The Commission is not competent to rule directly as a matter of fact that a disputed name belongs to a particular taxonomic species, because they may be wrong, and they may not force any taxonomist to accept as a fact what he believes is not a fact. However, the same desired result can better be attained indirectly. They can cancel any existing type material. This paves the way for recognition of a neotype in the sense desired. That done, no question longer remains of whether the original author did or did not describe a particular taxonomic species. The neotype thereafter determines the application of the name. For the same reason Conclusion 11(2) (*g*) (3) B.Z.N. p. 76, affecting Article 31, is wrong in principle and must be changed if it implies a direct

taxonomic decision. It is perfectly correct if the Commission acts by either suppressing the name, cancelling the type, or recognising a neotype.

19. See my comments on Article 31, on lectotypes and on neotypes.

20–28. *Type Locality.* I am perplexed by the Secretary's discussion of this topic. What is a type locality? We must agree upon a definition before our minds can reach a common groove.

To my mind (but clearly not the Secretary's, who may perhaps reflect a view peculiar to Lepidopterists) the type locality is the spot on the face of the earth where the holotype, lectotype or neotype was living a natural life (unaffected by artificial transport) at the time of its capture. If it were a bred specimen, it would be the spot where its nearest free ancestor was captured. That spot of "locality" can only be known to the person who "collected" the specimen in nature, or from some record left by and derived from its captor, and in many cases cannot be known at all, because no record was kept, or if kept has been lost, or an erroneous record substituted. It is, however, a matter of fact, of truth, just as much as the size of the type, or the number of scales or something else that it possesses is factual. The only method of determining the size is to measure the type, of the number of scales is to count them. These things cannot be determined by the decision of a commission, no one of whom ever saw the type itself.

The Secretary's views appear to be that the type locality is a nomenclatorial fiction, divorced from reality, that a commission may establish.

In paragraph **26** we read "a species based upon an erroneous type locality occupies a position very similar to that of a genus based upon an erroneously determined type species." But a species is not based upon a type locality, it is based upon a type specimen and solely that. It is impossible to have *two* criteria for determination of the application of a trivial name.

Species and subspecies are, by the *Règles*, co-ordinate, and whether a species is polytypic with two or more subspecies, or each of these populations is a distinct species is purely a matter for taxonomy to decide. It is a matter upon which taxonomists hold diverse views. When the holotype, lectotype or neotype determines the taxonomic form to which a specific name belongs it equally determines the form which is the potential nominotypical subspecies. For those who regard the species as polytypic, it is the nominotypical subspecies. Each of the other subspecies (which may be species in the eyes of some taxonomists) has its own type and from the standpoint of nomenclature must be regarded indifferently as species or subspecies.

There may be subspecies individual specimens of which can be differentiated only by those taxonomists who know where the specimen came from. Usually we expect subspecies to be sufficiently distinct for the taxonomist to be able to perceive from where the individual did come. That is not always the case because the criteria of differentiation may regard percentage of the population, rather than individuals. In any case it is purely a taxonomic problem. If nomenclature tries to solve it or to set up artificial standards, it will get its fingers burned.

What then to do, if we have a type with no record, or with an incorrect record, of the locality from which it emanated? What, one might equally

ask, to do if we have a type that has lost its head, where all differentiating characters are to be found ? Certainly we cannot, in the latter instance, permit any commission to rule that the type had six, or seven supraorbital setae, or what not. No commission saw the type, none knows, and they probably would not have been capable of making an *objective* determination if the unmutilated type were before them. Equally, in the former instance, they cannot establish a fictional locality, for the taxonomist wants fact, not fiction, and especially not fiction established arbitrarily by those who know nothing about the taxonomy of the group concerned. In either case, we are dealing with an imperfect type specimen, and if that imperfection is so great that taxonomists in the group concerned find it useless as a criterion for correctly applying a specific (or subspecific) name, they may seek relief by petitioning the Commission to cancel the type, and thus to pave the way for the establishment of a neotype which has preserved not only the necessary physical characters, but an accurate record of its place of capture, which will then be, without arbitrary action, the factual type locality.

A type locality is not, or is only rarely, essential to the interpretation of a species (subspecies). The fact to be filled in by taxonomists as rapidly as possible is the area and physical conditions within the area that the population inhabits in nature. The center of abundance is much more important than the incidental spot where the type was captured. But none of this concerns the nomenclaturist at all until (occasionally) the point is reached where the taxonomist can not determine his material until he knows where it came from. It follows that the usefulness of stating a type locality, if factually correct, is to serve as a check on the geographic differentiation of populations. If fictional and erroneous it would have to be disregarded by any taxonomist as soon as the fact became known.

Two instances will illustrate the absurd results that could and would arise if the Commission were empowered artificially to designate type localities *without cancelling the status of the type specimen :*

(1) Paragraph **26,** deals with type localities originally erroneously cited. If the actual locality is unknown, it is suggested that the Commission shall, under certain circumstances, designate whatever locality they consider to be most appropriate.

A species of *Campsomeris* was described as from China. The Oriental species of *Campsomeris* have been critically studied by a recent author who recognised the fact that no such species occurs there. That author listed it as a *species inquirenda.* Suppose that, on the contrary, wishing to clear his list, and thinking to have recognised the form in an African species, although he had no critical knowledge of the Ethiopian fauna, he had recommended to the Commission that the type locality be designated " Africa." There would at that time have been no taxonomist who knew anything to the contrary, and the Commission (had the proposed rule been in effect) would doubtless have acceded.

Actually the type is a specimen of a common South American species, a fact that no one but the writer happens to know, and that by reason of having studied the type and compared it in the South American material. Would he be keeping faith with truth and science if he were to accept the arbitrary ruling that the type locality (and therefore the holotype) were African ? In this

instance there is no need to establish the type locality. When the writer was able to identify the holotype with an actual living species, he was able to state the geographical distribution of that species, and where the holotype happened to have been caught was not even of academic interest.

(2) In paragraph **23**, suggested rule (7), it is proposed in brief that where the type locality has not been designated or indicated it shall be the first subsequently selected as such.

Let us assume a species " a " described from New York, Carolina, Jamaica. The holotype has no indication as to which place it came from. Three subspecies are known to exist, one inhabiting the Appalachian Mountains (hence including New York State and the mountains of North Carolina), one the coastal plain (hence including Long Island in New York) and the third the West Indies. A taxonomist, concluding that the holotype could have been any of the three, sets Jamaica as the type locality. Later taxonomists, with more precise discrimination, are able to definitely determine that the holotype belongs to the Carolinian coastal plain form. Then we have the type continental, the type locality insular, where the subspecies represented by the type does not occur. That is the sort of absurdity that may result from establishing two criteria that may conflict for application of a name.

From such considerations I cannot support the proposed rules, which appear to me a sort of shadow-boxing, a method of setting up taxonomic fictions to pose as fact, and which in the end do not concern nomenclature.

29 and **30.** *Fossil Species : Parasitic Species.* The principles conform with the preceding and the same remarks apply.

34–37. *Official Lists. Nomina conservanda.* Perhaps some feeling exists that these are intended only for very widely applied names and not for the host of minor names known chiefly to specialists. A strong publicity drive might be very successful in enlarging the *List* rapidly.

In the Hymenoptera lists of all generic names with type-species have been issued for Chalastogastra, Ichneumonoidea, Chalcidoidea, Cynipoidea, Ants, Pompilidae and Bees. All establish the types believed by the authors to be correct under the *Règles* as then formulated. If they could be re-examined and changed wherever contrary to current interpretations of the *Règles*, and cases at variance with continuity brought to the attention of the Commission for decision, we could soon add the major block of generic names employed in this great order to the *List*. It would be an example for others. All the names in " Generic Names of British Insects," if incorporated into the *Official List* would themselves make an even greater block.

I have also long felt, that in cases where all generic names in a group are too numerous for any taxonomist or group of taxonomists to handle, that the important results desired could be largely attained by a study of all generic names up to about 1825 or some other period varying according to publication of some early basic work in the particular field involved. Here again a little publicity might instigate such investigations, and the mere fact that the results would be incorporated in the *Official List* would be a strong stimulus. People may say that it is extremely difficult to be sure one is right in a large list of old names. But correctness in every detail is far less important than a definite decision for the future, in conformation with usage where that has been clearly established.

Annexe to Statement furnished by Professor J. Chester Bradley

NOTES TOWARD AN INTRODUCTION TO THE INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE

These International Rules of Zoological Nomenclature are a set of laws designed to govern in an orderly manner the application of names to all of the categories involved in the classification of animals. They are enactments of the successive International Congresses of Zoology, but their authority lies only in the extent to which they interpret and express the will of zoologists in whose conscience their enforcement lies. The penalties for their fracture are not material but are expressed in the disservice done to zoology.

While based on principles, they recognise none as paramount to their fundamental aim, which is to provide the maximum stability and continuity in nomenclature compatible with freedom of taxonomic thought. They seek to provide *the* name which every zoologist, under whatever circumstances may be imposed by his personal taxonomic judgment, shall apply to any given kind or group of animals. They especially seek to provide that, under the same circumstances, that name shall be permanently the same.

They refrain from impinging upon taxonomic judgment, which must not be made subject to regulation nor restraint. Harmony with taxonomy, however the latter fluctuates, is secured by the device of types. Each name is conceived to be based on a type of inferior category, which for nomenclatorial purposes defines it objectively. Thus the name of a species is ultimately defined by the characters of an individual—its type, that of a genus by its type-species, that of a family by its type-genus. From the viewpoint of nomenclature species, genus or family consists each of its type plus all the other individuals, species or genera that any given taxonomist holds to belong to it. The limits of each are questions of taxonomy, ignored by nomenclature. Nomenclature accepts as objective synonyms only those units that are based on the same type ; but it is prepared at the same time to accept or to reject subjectively as synonyms units based on other types, in the sense that it provides the proper name for the taxonomist to use, whichever course his taxonomic judgment prescribes.

Equally nomenclature does not determine the rank accorded to any group of organisms, but it does provide the name that shall be applied to whatsoever rank any taxonomist may wish to assign it.

From these considerations it follows that the complete binomial name of a species can be stabilised only for the type-species of each nominal genus, since the generic placement of all others is a matter of fluctuating taxonomic judgment.

Conceiving nomenclatorial rules as tools useful only to the point where they provide the maximum stability compatible with taxonomic freedom, certain measures have been adopted to prevent their becoming tyrannical, and actually destructive of their own usefulness.

The first of these provides that the rules may be suspended by an authorised body in any case where their operation would cause change and confusion.

The second provides that where practice has established a certain name under the International Rules as they existed in 1907 or later, that name shall not thereafter be changed to conform to subsequent revision of the Rules.

The third provides that long established usage shall, under certain safeguards, take precedence over priority in case where application of the latter principle would overthrow such usage.

The fourth provides that where an author discovers that a well-established name must suffer a serious change under the rules, he shall bring the matter to the attention of the International Commission on Zoological Nomenclature for a ruling before instituting such change.

(Note : Provisions 2 and 3 above are " wishful thinking." They are tentatively inserted with the conviction that such rules should exist, and the hopes that they will.—J. C. B.)

DOCUMENT 1/51

Statement of the views of the ENTOMOLOGISCHE GESELLSCHAFT, BASEL

Extract from a letter dated 30th July 1952 from M. HENRY BEURET

Sur ma demande vous avez eu la grande obligeance de m'envoyer les parties 1–8 dy vol. 7 du *Bulletin of Zoological Nomenclature* contenant divers problèmes de nomenclature ainsi que les déductions et propositions qui devraient être soumises au Congres international qui aura lieu l'année prochaine à Copenhague.

Tout d'abord je tiens à vous remercier cordialement d'avoir bien voulu me transmettre ces publications et ensuite à vous féliciter chaleureusement pour l'immense travail que vous avez accompli en étudiant à fond les problèmes qui nous interessent.

L'Entomologische Gesellschaft, Basel, qui s'intéresse vivement à toutes ces questions de nomenclature a étudié vos exposés et m'a chargé de répondre aux diverses questions que vous avez posées.

Je suis heureux de vous dire d'emblée que, dans les grandes lignes, nous voyons la résolution des divers problèmes sous le même angle que vous-mêmes. Comme nous avons un grand intérêt à ce que le Congrès de Copenhague fasse du bon travail, nous n'avons nullement l'intention de créer des difficultés mais nous voudrions plutot *vous appuyer dans vos efforts*. C'est dans cet ordre d'idees que nous avons examiné vos propositions ; nous ne ferons donc que les remarques suivantes :—

Pages 152 et suivantes :

No. 1– 3 Rien à objecter.

No. 4– 7 Une " law of prescription " est dangéreuse et nullement necéssaire pour arriver à une solution satisfaisante du problème soulevé sous No. 1–3 !

No. 8–15 Rien à objecter.

No.16–17 D'accord, mais le terme " General body of workers " devrait à notre avis être mieux précisé.

No. 18 D'accord. (" general body of workers " éventuellement à preciser).

No. 19–22 Rien à objecter.

No. 23

Rules a, b, c, d, e, f: D'accord.

Rule g: Si *l'auteur de la description originale* choisi ultérieurement *lui-meme* une localité comme " type locality " parmi celles qu'il avait mentionnées dans la description originale, nous ne voyons rien à objecter.

si un *autre* auteur fait ce choix, n'y aurait-il pas lieu de l'obliger de choisir la *première* localité mentionnée dans la description originale? En effet, il nous semble que la localité citée en premier lieu a plus de poid, ce qui ressort dailleurs très souvent des descriptions originales, sans que leurs auteurs aient expressement mentionné qu'il s'agit là de la " type locality."

Doit-on admettre que *l'auteur* d'une forme ou une autre personne puissent ultérieurement choisir comme " type locality " une localité *qui ne figure pas parmi celles mentionnées dans la description originale*? (Le cas n'est pas exactement le même, lorsque la description originale ne mentione aucune localité, ou seulement une indication très imprécise comme par exemple " dans les Alpes " !)

No. 24 Rien à ajouter.

No. 25 Why only a " Recommendation " and not a *rule*?

No. 26–28 Rien à objecter.

No. 31–37 Rien à objecter.

No. 39 (1) " Law of Prescription " : **Non !**

(3) " stability by means of developments carried out within the existing *Règles* " : **Oui !**

. . . . . .

Dans vos propositions vous utilisez souvent les termes suivants " specialist " ou " general body of workers." Si ces expressions doivent être admises dans le texte officiel des *Règles*, il y aurait, nous semble-t-il, lieu de donner une définition précise de ces expressions. Qui est " specialist " ?

Lorsque les " specialists " ne seront pas d'accord sur un problème quelconque, est-ce l'opinion de la majorité qui décidera ? Il se peut fort bien que suivent la minorité ait raison.

En ce qui concerne les *noms douteux*, nous estimons qu'un tel nom cesse d'être douteux si par n'importe quelle méthode on arrive à reconnaitre avec certitude la forme désignée ou décrite sous ce nom.

J'espère vivement que le grand travail que vous avez accompli soit suivi par des décisions qui marquent un grand pas vers l'assainissement complet de la nomenclature.

Extract from a letter, dated 13th August 1952, from Francis Hemming, the Secretary to the International Commission on Zoological Nomenclature, to Henry Beuret

The point raised on the last page of your letter in regard to the meaning of such phrases as " specialist " requires, I agree, careful consideration. In the context in which these phrases were used in my papers in volume 7 of the *Bulletin*, this expression was intended to mean that what was wanted in regard to whatever might be the subject under consideration was a statement of the views of specialists (=workers in, or students of) the group concerned ; it was intended to exclude comments from zoologists whose work was not directly affected by the question at issue. I fully agree that, if any expression of this sort is used in the *Règles*, it must be so used that its meaning is entirely unambiguous. So far as the *Règles* are concerned, this problem of drafting arises chiefly in connection with such questions as the use of the plenary powers, the determination of *nomina dubia* and the like. Here it is the views of persons concerned with the group and the views of no one else which are directly relevant. On the question which you also raise as to the position when in any given group some specialists hold one view and others another, what I had in mind was that any interested specialist in the group concerned should be at liberty to put his views before the Commission and that the Commission, after taking into consideration any comments, either for or against the proposal in question, which might be elicited by the " advertisement " by the Commission of the proposal concerned, would itself decide what action ought to be taken. Reverting to the first of your two points, I think that it will be important to avoid using in the *Règles* any expression in this connection which might later be interpreted in too restrictive a sense. By this I mean that, while it is important to secure the general feeling of specialists in any given group, it is often important also to take account of the views of other interested workers, e.g. workers in the fields of applied biology who may be deeply concerned with the maintenance of a given name but who are not " specialists " in the particular group in question, so far as its systematics are concerned. Moreover, no expression should be used which implied that, for a problem arising in a given group, the only persons whose views were desired or who were authorised to raise a matter with the Commission were professional zoologists working on that group, for it often happens that independent zoologists (i.e. what in this country we call by the rather misleading title of " amateur ") have an important contribution to make.

DOCUMENT 1/52

By W. E. CHINA, D.Sc.
(*British Museum (Natural History), London*)

Extract from a letter dated 20th August 1952

REF. Z.N.(S.)359 (STABILITY IN ZOOLOGICAL NOMENCLATURE) ANSWERS TO QUESTIONS ON PAGES 187–188 (PARAGRAPH 39) IN THE PAPER BY THE SECRETARY TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

1. I consider that a properly stated Law of Prescription would be of great help in stabilising nomenclature.

2. It should not be made retrospective otherwise all the changes in the 40 years which have now been more or less generally accepted will be invalidated and a further series of changes be necessary. It should become operative at some future date to be advertised. Ideally each case should be reported to the Commission and the old names discovered should form part of the Official List of invalid names.

3. At the same time developments within the existing framework of the *Règles* should be promoted and would form an additional safeguard for future work.

4. One of the factors bringing about instability in names which is not dealt with in your report is the uncertainty of the exact date of many publications e.g. Laporte's Easai Class Syst. Hemipt. 1832 or 1833. Research into dates often brings about a change of date which results in changes of generic and even of family names. I suggest that the Commission should publish a complete list of literature prior to 1900 giving (arbitrarily if necessary) official dates of publication and at the same time indicating those works which are officially regarded as invalid.

5. As an example of an entirely new scheme, one of the novel expedients mentioned in paragraph 3, I propose that which has long been suggested and discussed by workers at this Museum. It is the proposal that there should be an International Museum and that no species should be valid unless the type is deposited in this museum. The law of priority would hold only so far as type specimens were available. Species without types would be invalid. Until such time as the International Museum could be established, its place would be taken by the National Museums. Each country would maintain specialists at the International Museum who would gradually built up authoritatively named collections for their own National Museum. This scheme would have the advantage of close contact between specialists of various nations working on the same group, and the elimination of the majority of dubious species described in the old literature. If necessary, neotypes could be established in this International Institution for all those older specific names which it was desirable to maintain.

Extract from a letter, dated 19th September 1952, from Dr. W. E. China to the Secretary to the International Commission on Zoological Nomenclature

I agree about the difficulty of enunciating a satisfactory Law of Prescription to cover all Classes and Orders. It would almost certainly be necessary to have a different starting point for each group, to date from the first real monograph of the group, this work to be agreed upon by the specialists in that group.

Another method would be to advance the starting point of Zoological Nomenclature from 1758 to say 1900, giving the workers operating at that date the authorship of all the pre-1900, names. This of course would create a precedent and would deal a death blow to the law of Priority since similar " purges " could be expected every few generations when the nomenclature again became chaotic.

Official List of publications :

I should prefer a list, to be compiled by someone with the assistance of specialists, of all the taxonomic works, say up to 1900 which are to be officially regarded as valid*. The generally accepted dates should be given unless already proved to be otherwise, and all dates on the list arbitrarily fixed. I would suggest allowing a few years in which changes could be made during which specialists should be encouraged to make application for the addition of titles and alteration of dates. After that no change in the list should be allowed on any account.

I can think of no one, other than yourself, with sufficient energy and enthusiasm to compile such a list.

International Museum : I agree that this will be impracticable for many generations but the same idea could be adopted using the leading National Museum. Private collectors would, naturally be against such a scheme, but what I am anxious to do by this method is to invalidate all the old species based on lost or doubtful types. This would result in a more rational and less unstable nomenclature. At the moment the types of very many species are lost or the location unknown and such species are really based on unofficial neotypes in such institutions as the British Museum and other National Museums where identifications have been continually circulated for many years. The real types are the " neotypes " not the specimens actually seen by the original authors. My scheme would merely legalise the present set-up.

*This list could be based on Sherborn up to 1850.

DOCUMENT 1/53

By the
NOMENCLATURE COMMITTEE OF THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Enclosure to a letter, dated 28th August 1952, from Dr. ERNST MAYR

We propose that in the ruling, based on the Monaco Resolution, which grants Plenary Powers to the Commission, the wording should be altered to provide that these Powers are to be used for the purpose of preventing confusion and of promoting uniformity and stability in zoological nomenclature.

Signed :

The Nomenclature Committee at the American Museum of Natural History, New York,

ERNST MAYR,
G. H. H. TATE,
JOHN T. ZIMMER,
C. H. CURRAN.

DOCUMENT 1/54

By K. H. L. KEY

(*Commonwealth Scientific and Industrial Research Organisation, Canberra, Australia*)

Extract from a letter dated 4th September 1952

I have studied your proposals in connection with the other six questions referred to you by the Thirteenth Congress,* and in general I find myself in hearty agreement with them. I do not like the definition of " subspecies " adopted by the Thirteenth Congress ; I hope to be able to submit a short paper on this later on.

DOCUMENT 1/55

Statement furnished by the

AMERICAN SOCIETY OF PARASITOLOGISTS

Extract from a letter dated 8th September 1952

The American Society of Parasitologists at its meeting in November of 1951 appointed a committee to investigate the controversy that arose following the announcement that " far-reaching decisions in regard to zoological nomenclature (had been) taken by the Thirteenth International Congress."

In undertaking its assignment the *Bulletin of Zoological Nomenclature* was studied and the requests for advice in Volume 7 were noticed. The committee reached a unanimous decision on only three points.

. . . . . .

(3) The committee agrees that a preamble to the International Rules such as you suggest would be valuable.

In submitting these comments to you the Society wishes to express its gratitude to you and the entire International Commission for the considerable effort that has been expended on behalf of zoological nomenclature.

Signed for the Society by its Committee,

ALLEN McINTOSH,
D. H. WENRICH,
G. W. WHARTON, Chairman.

*i.e. the questions other than that relating to the emendation of names on which Dr. Key submitted a separate statement. This will be published later in the " Document 5 " Series.

DOCUMENT 1/56

By JOSHUA L. BAILY, Jr.
(*San Diego, California, U.S.A.*)

Extract from a letter dated 8th January 1953

Stability and the Plenary Powers

As I have so often said to you, the most helpful step you can take in stabilising nomenclature is to suspend the rules whenever necessary to preserve a name universally understood, where the application of the rules would compel recognition of a name that has never been used.

DOCUMENT 1/57

Statement furnished by the WIENER ENTOMOLOGISCHE GESELLSCHAFT

Letter from Dr. HANS REISSER, dated 7th March 1953

Editorial Note: This letter is concerned mainly with an individual case (that of the name *Papilio adippe* Linnaeus), but is included here because in it the Society states its view on the general question of the use of the plenary powers.)

By our member, Mr. Schwingenschuss, we have got your inquiry from 20th September 1952, about the question of nomenclature of the species *Argynnis adippe* L. We beg to excuse the delay in answering.

Of course we agree with great pleasure that the name of *adippe* should be conserved. This case illustrates that the application of the strictest priority and exhumations of obsolete names only produces such a trouble that it becomes necessary to use the vulgar denominations instead of the scientific ones in order to signify the real species meant! Our Society would prefer the application of Heikertinger's " principle of continuity " and we should be very glad if it would become possible for a resolution of the authorities, treating with nomenclature problems, to introduce this principle into practice.

DOCUMENT 1/58

By CYRIL F. DOS PASSOS, LL.B.

(*Research Associate, The American Museum of Natural History, New York*)

Statement enclosed with a letter dated 2nd April 1953

Shall the " Règles " be amended so as to regulate the fixation of type localities and if so upon what terms and conditions?

Introduction

This problem has been considered recently in a very able review of the subject by the Secretary of the Commission (1952, *Bull zool. Nomencl.* **7**: 172–180), as part of a general study on the means to be found for promoting the greatest possible stability in zoological nomenclature. Secretary Hemming concludes that instability in nomenclature at the species-name level arises chiefly out of the lack of provisions in the *Règles* for determining the locality to be accepted as the type locality of a given nominal species, and he suggests provisions for putting an end to instability for this cause in the names of subspecies due to inadequate information regarding type localities. Most of the Secretary's recommendations cover recent species, but he has also considered parasitic species and fossil species. Most suggestions, it is believed, will meet with the general approval of zoologists.

The fixation of type localities is not a new procedure in the zoological sciences. It has been the well-established practice of entomologists, mammalogists, ornithologists, and probably other zoologists for many years. My colleagues, Dr. George H. H. Tate, Curator, Department of Mammals, and Dr. John T. Zimmer, Curator, Department of Birds, both of the American Museum of Natural History, have kindly furnished me with many examples of this procedure in their respective branches of zoology. In mammalogy may be mentioned papers by Merriam (1901), Allen (1916), Kloss (1921), Sanborn (1930), and Hershkovitz (1947). In ornithology there is a lengthy paper by Zimmer (1951) in which many type localities are suggested and fixed. In entomology it is necessary to refer only to a paper by Comstock (1944), one of my colleagues in the Department of Insects and Spiders of that institution, and to one by the present author in collaboration with Grey (1947).

Some zoologists have objected that in a small percentage of cases the fixing of type localities is a nomenclatorial fiction. That may be true where no type locality is given in the original description, or where a type locality is erroneous. But those situations demand correction, even if the remedy is fictional. There are other fictions in nomenclature. The lectotype designated to replace a holotype that has been lost or destroyed may, in a small percentage of cases, be another species! But by selecting it stability is obtained, because the name is tied to an object. The same may be said of neotypes, if authorised by the coming Congress. Stability in nomenclature cannot be obtained until many type localities are fixed, and is so important that the possibility that some errors may occur, or a few fictions result, cannot be allowed to prevent its attainment.

It does not seem necessary to the present author to argue too strenuously for rules governing the fixation of type localities. The reasons for incorporating such rules in the Code has been presented fully and ably by the Secretary in paragraph 22 (1952, *Bull. zool. Nomencl.* **7** : 172–173). It seems necessary only to add that zoologists for many years have fixed type localities from sheer necessity in their systematic work when dealing with polytypic species, because without fixing the type locality of the nominotypical subspecies, how can any other subspecies be named without the risk that one of them will be a synonym of the first subspecies named? Which that one may be will be uncertain until the type locality of the oldest name has been fixed.

The problems before the International Congress of Zoology in 1953 in studying this subject are to determine first, whether provisions for the fixation of type localities shall be incorporated in the *Règles*, and if so, secondly, what these provisions shall be. This paper will consider the second problem, because the first seems to have been pretty well settled, and the main efforts should be directed to the codification of the present practice.

Preliminary Considerations

The phraseology used by the authors mentioned above in fixing type localities has not been uniform, and in a few cases could have been more definite. For instance, Comstock (1944 : 541–542) states " Therefore . . . is hereby fixed as the type locality of . . . " while Zimmer (1951 : 6, 16) has used a variety of expressions ranging from " For this reason I have selected it as the restricted type locality for . . . " to " I suggest . . . " to cite two examples only. Perhaps if rules covering this subject are adopted, it would be well to provide for a definite formula to be used in the future, with a somewhat milder formula for those type localities already selected.

Furthermore, different words have been employed from time to time by authors in fixing type localities, and it would be well to select one for the rules on this subject. Some of those appearing in the literature are " designate," " fix," " restrict," " select," " suggest," etc. In view of the fact that " designate " or " indicate " have been used in connection with a holotype (1950, *Bull. zool. Nomencl.* **4** : 186), " designate," " indicate," and " select " with the type species of a nominal genus (1950, *ibid.* **4** : 179), " designate " with the type specimen of a species (1950, *ibid.* **4** : 187–188), " select " with a lectotype (1950, *ibid.* **4** : 186), and that " designate " has been proposed for neotypes (1952, *ibid.* **7** : 137), maybe it would be well to choose one of the other words. In the proposed rules hereinafter set forth, " fix " has been used, because it is rather usual to employ that word in the United States of America, but the only matter of importance is uniformity. Any of the other words would be equally suitable.

Consequently, I proceed to the formulation of proposed rules. Three different situations arise which should be covered by the *Règles*. First, where no type locality is mentioned in the original description, secondly, an erroneous type locality, such as India for a species that never occurred there, but does occur in America, and, thirdly, where the type locality mentioned is so indefinite

that present day knowledge shows it to be insufficient for all practical purposes, i.e. America, Europe, etc. All other cases where it appears necessary or desirable to fix a type locality would appear to fall within one of these three main divisions.

Credit for most of the proposals must be given to the Secretary of the Commission, but in the concluding paragraphs of this paper some instances have been pointed out in which our agreement is not complete. Certain drafting changes and omissions have been made also in the Secretary's proposed rules which, however, are not believed to affect their meaning substantially.

Proposed Rules for the Fixation of Type Localities

DEFINITION.—The type locality of a species, subspecies, or infra-subspecific form (all hereinafter included in the word " species "), shall be that place where the species was taken or found in nature (unaffected by artificial transportation), unless for good and sufficient reason, and in accordance with the provisions of the following rules; (1) an author shall select subsequently a different type locality for the species, or (2) the Commission, upon application, shall otherwise direct.

RULES.—(*a*) Where no type locality is given in the original description, an author may fix a type locality where the species did occur at that time.*

(*b*) Where no type locality is given in the original description, but the author cites bibliographical references which mention one or more localities where the species occurs, an author may fix one of said localities as the type locality of the species.

(*c*) Where the type locality given in the original description is erroneous (being a place where the species did not occur at that time), an author may fix a type locality where the species did occur at that time.

(*d*) Where an author in the original description indicates a single type locality, that locality, if not erroneous, is to be accepted as such.

(*e*) Where an author indicates several type localities, the locality where the holotype was taken or found, or from which a lectotype is designated, shall be accepted as the type locality.

(*f*) Where the label attached to the holotype, or as the case may be to the lectotype, gives a more precise indication of the locality where the species was taken or found than that given in the original description, the more detailed locality so given is to be accepted as the type locality of the species concerned, when a subsequent author publishes the particulars thereof.

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*It is a question whether a name proposed in an original description that does not contain a statement where the type was found should not be considered a *nomen nudum*, since a type locality is an essential ingredient of a good original description, but perhaps such cases are not very frequent or much worse even than those in which the type locality is entirely erroneous.

(*g*) Where an author finds that the fixation of the type locality as specified in Rules (*a*), (*b*), (*c*), (*d*), (*e*) and (*f*) is not sufficiently definite, he may fix the type locality more definitely by publishing such fixation with his reasons therefor, in which event the restricted type locality so fixed shall be accepted as such.

(*h*) The localities to be regarded as having been originally cited for a given nominal species shall be (i) the locality or localities cited in the original description of the species (including any more detailed locality ascertainable from the label attached to the holotype, or where no holotype was designated the syntype later selected to be the lectotype), or any restricted locality comprised within one of the localities aforesaid, and (ii) in default thereof the locality or localities indicated in any previously published work cited by the original author as applying to the species in question, but shall not include any locality only doubtfully cited by the original author, but preference shall be given to any localities given in earlier works by the author of the specific name in question when bibliographical references to such earlier works were cited in the original description of the species in question.

(*i*) When the original locality or one of the original localities cited by the author of the name of a given species is found to be incorrect, such other locality or localities indicated as being the locality or one of the localities in which the type material or part of it was obtained, is to be deemed to be such locality.

(*j*) When the name of a species is found to be invalid and a new name is proposed for it, the nominal species so established shall have the same type locality as the nominal species for which it has been established as a substitute.

(*k*) Where the type locality of a species has not been fixed under any of the preceding rules, the type locality of that species shall be whichever of the originally included localities is first definitely fixed as such at a later date, either by the original author or by any other author.

(*l*) When after a type locality has been fixed under the foregoing rules it is found that the restricted type locality so selected is not sufficiently precise, it shall be open to an author to restrict further the type locality of the species concerned by fixing some place or area comprised within the previously selected type locality to be the restricted type locality of the species in question, and if necessary a succession of restricted type localities may be fixed, each situated within the area comprised in the "type locality" last previously selected.

(*m*) In fixing the type locality of a parasitic species, the species to be accepted as the host species shall also be determined for nomenclatorial purposes.

(*n*) In fixing the type locality of a fossil species, the geological age of the rocks and the horizon in which the species was found shall also be determined for nomenclatorial purposes.

(*o*) If subsequent to the fixation of a type locality new evidence is discovered, either from the rediscovery of the holotype, syntype, or from manuscript notes showing the true type locality of the species in question, the matter shall be referred to the Commission, which shall have power to fix the type locality.

(*p*) A type locality fixation made prior to the adoption of these rules, if in substantial compliance therewith, and evidencing an intent on the part of the author to fix a type locality, shall be recognised as valid.

(*q*) After these rules take effect, the expression " fix a type locality " is to be strictly construed, and to exclude the mere mention on a later occasion of a locality as one in which the species occurs.

(*r*) The Commission, upon application, for good and sufficient reason, may cancel or alter the fixation of any type locality of a nominal species made by an author hereunder, or prior to the adoption of these rules, except where the original author cites one type locality only, unless that locality is erroneous, and fix a new type locality.

(*s*) Whenever the Commission is requested to fix a type locality, or otherwise act hereunder, it shall give public notice of the receipt of such application in like manner as that prescribed in cases involving the use of the plenary powers, and shall prescribe that a period of twelve months shall elapse from the date on which such public notice is given and the date on which the Commission may take its decision thereon.

(*t*) In the fixation of a type locality, the Commission shall select one which harmonises best with current nomenclatorial practice, except where the Commission on the advice of specialists is of the opinion that such practice is erroneous and that its perpetuation would lead to confusion, in which event the Commission may designate as the type locality of the nominal species in question whatever locality it may consider to be the most appropriate.

Recommendation.—Authors are urged in their original descriptions to fix the most precise type locality possible for a species, and to give at least the name of the country, state, or province, as well as the county or district, and the city, town, or village where the species was taken or found, and if the locality is remote to give the latitude and longitude. It is also advisable for authors to add the altitude and describe briefly the faunal zone. In the case of parasites, the name of the host, if known, should be given, and in describing fossil species the name of the geological formation should be added.

Conclusion

In preparing the foregoing rules an effort has been made to keep them as simple as possible, and to cover most foreseeable contingencies. Also, it has been assumed that the primary responsibility for fixing type localities should rest upon the author. Recourse to the Commission should be had only when authors find themselves in disagreement. However, at that point full and complete authority should be vested in the Commission to solve all the problems submitted to it as it may believe best. We now proceed to a few matters in which I do not find myself in complete accord with the Secretary's proposals.

I do not concur with the Secretary's Rule proposed in paragraph 23 (*d*) (type by [absolute or virtual] tautonymy) (1952, *Bull. zool. Nomencl.* **7** : 174), because such type localities as those given as illustrations, i.e., *zermattensis*, *adriaticus*, and *altaianus*, are too indefinite to be of much value, and sometimes similar type localities are false, or at least misleading, i.e., *britannicus* for an Irish insect (1950, *ibid.* **3** : 115). In one case the name *Argynnis atlantis canadensis* was proposed for a Newfoundland insect by the author of the present paper (1935 : 85), not because the insect occurred only in that country, but because it occurred in that faunal zone. In another case the name *Parnassius phoebus manitobaensis* was proposed by Bryk and Eisner (1935 : 55), and the type locality was given as " Manitoba, End-Mountains," but there are no " End-Mountains " in Manitoba, nor does the insect occur in that province of Canada. However, there are End Mountains in Alberta, where the insect does occur. These few illustrations show the danger of having anything to do with tautonymy in fixing type localities. Consequently, no rule has been drafted to cover such cases.

Neither do I agree with the Secretary's suggestion (1952, *Bull. zool. Nomencl.* **7** : 175) that a rule governing the subsequent fixation of a type locality shall provide " . . . that no locality can be validly selected as the type locality of a species, if it was not included among, or comprised within one of, the localities cited at the time when the species in question was originally described." Such a rule would prevent the proper fixing of some place in North America as the type locality of an insect erroneously described from India, where it never did occur, i.e., *Papilio genutia* Fabricius, 1793. However, the Secretary's proposal would be quite proper if it were qualified, as he may have intended, by substantially the following phrase " except when no type locality was cited in the original description, or that locality was erroneous."

It does not seem necessary, as proposed by the Secretary in paragraph 26 (1952, *Bull. zool. Nomencl.* **7** : 178) to provide for the case when one of the original localities cited by the author of the name of a given species is found to be incorrect, because the other correct locality would govern such a case. It is to be assumed that an author would not fix an incorrect type locality.

It is accordingly urged that the *Règles* be amended by the Fourteenth International Congress of Zoology at Copenhagen in 1953 so as to include a new article governing type localities. The suggestions herein made are offered as a basis for the discussion of that problem.

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Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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CASE NO. 2

DOCUMENTS RELATING TO THE QUESTION WHETHER "NEOTYPES" SHOULD BE RECOGNISED IN THE "RÈGLES" AS A CATEGORY OF TYPE SPECIMENS

DOCUMENT 2/1

Origin of the Present Investigation

Editorial Note.—The present investigation into the question whether "neotypes" should be recognised in the *Règles* as a category of type specimen was undertaken in response to an invitation given in July 1948 when the International Commission on Zoological Nomenclature at its Paris Session had under consideration a suggestion received from Dr. Don L. Frizzell and Dr. Harry Wheeler on the subject of the recognition of neotypes (see Document 2/2 below). The Commission then agreed (see 1950, *Bull. zool. Nomencl.* **4** : 192) :—

"(*a*) that the proposal to recognise the category 'neotype' raised complex problems which required much closer and more detailed study than had yet been given to them ;

" (*b*) that, if the Congress were to be recommended to recognise the category ' neotype,' it would be essential that the proposals so submitted should be comprehensive in character and should contain adequate safeguards against the abuse of the new provision by mercenary or irresponsible persons ;

" (*c*) that, in view both of the intrinsic difficulties involved in the proposed recognition of the category ' neotype ' and of the wide differences of opinion on the subject which at present existed among zoologists, it was essential that further discussions should be held with interested groups of specialists before the Commission submitted any recommendations to the Congress for the amendment of the *Règles* to deal with this subject."

2. The Commission further agreed to recommend (1950, *ibid.* **4** : 192-193) " that the Secretary to the Commission should be invited to make a thorough study, in conjunction with interested specialists, of the problems involved in the proposal that the category ' neotype ' should be recognised in the *Règles* and to submit a Report thereon, with recommendations, for consideration by the Commission at their meeting to be held during the next (XIVth) meeting of the Congress, with a view to the submission by the Commission of a considered statement of their views on this subject and, if they decided in favour of recommending that the foregoing category of type specimen should be recognised in the *Règles*, of a comprehensive scheme to that end."

3. The foregoing recommendation, with other recommendations, was submitted to, and approved by, the Section on Nomenclature of the Thirteenth International Congress of Zoology at its Second Meeting held on 24th July 1948 (1950, *Bull. zool. Nomencl.* **5** : 71, 76).

DOCUMENT 2/2

General Consultation With Specialists and Issue of an Appeal for Advice in 1952

Editorial Note.—Attention is drawn to the review prepared by the Secretary to the International Commission on Zoological Nomenclature of the problems involved in the question whether the category " neotype " should be recognised in the *Règles* as a category of type specimen which was published in March 1952 (*Bull. zool. Nomencl.* **7** : 131-147). This review contained an appeal to specialists to assist in the present investigation by furnishing statements of their views on the action which it was desirable should be taken. It was in response to this appeal that the majority of the documents now submitted was furnished.

DOCUMENT 2/3

The question whether " neotypes " should be recognised as a category of type specimen was first brought before the International Commission on Zoological Nomenclature by DON L. FRIZZELL and HARRY E. WHEELER in a paper entitled " On the Question of recognising ' Neotypes '." This paper was published in August 1945 (*Bull. zool. Nomencl.* **1** : 106-108).

The stand taken in the above paper is shown by the following extracts therefrom :—

> In a case in which the original types of a species " A " are lost or otherwise unavailable, and in which two or more species—all fitting the description of species " A "—occur at the type locality, can the identity of species " A " be established by the designation of one or more neotypes ? . . . If the designation of " neotypes " is not accepted, how is the identity of species " A " to be determined ? The following case is submitted to the Commission in the hope of obtaining a ruling on the validity of the term " neotype " and the principle involved, rather than merely to solve the specific problem offered by the following example. . . .
>
> If the holotype of a composite species such as . . . were in existence, there would be no doubt that it would represent the species to which the original name must be attributed. When the original types are completely unavailable, however, the reviser must select arbitrarily the species to bear the name. If at the same time he can designate a " neotype " (or " neotypes," if " neoholotype " and " neoparatype " can be admitted) which will be accepted by the Commission as they now accept holotypes, such procedure will certainly tend to stabilise nomenclature and prevent some of the shuffling about of names which is common at present.

DOCUMENT 2/4

In a paper published at the same time as that by Frizzell and Wheeler, FRANCIS HEMMING, Secretary to the International Commission on Zoological Nomenclature, drew attention to certain abuses which might arise if the " neotype " concept were to be incorporated into the *Règles*, unless at the same time stringent safeguards were laid down (Hemming, 1945, *Bull. zool. Nomencl.* **1** : 108-111).

DOCUMENT 2/5

By H. S. BARBER

(*United States Department of Agriculture, Agricultural Research Organisation Bureau of Entomology and Plant Quarantine, Washington, D.C., U.S.A.*)

Letter dated 23rd June 1947

The enclosed article* was written before I saw your discussion of "neotypes" in your *Bulletin of Zoological Nomenclature* (Vol. 1, No. 5, p. 108 (Z.N.(S.)24)).

I protest that :—

(1) The neotype question is one of taxonomic practice and not one of nomenclature.

(2) Questions of nomenclature begin and end in such published literature as complies with the Code.

(3) Zoological questions are problems for taxonomic research and are as yet not covered by rules authorised by the International Congresses.

(4) Since type specimens can never be published (although they are recorded in publications) they are not nomenclatural (as in 2) but are subjects of zoological research (as in 3).

(5) Neotypes are proposed by zoological analysts for consideration by their colleagues and successors. These colleagues and successors will evaluate their significance as they do other units of evidence bearing on identification of named kinds.

(6) Your Commission has no jurisdiction over processes of zoological analysis, including neotype establishment, but may rule on validity of published nomenclature.

(7) As example of neotype proposal for solution of a problem of zoological identity I would cite and here include part of my published discussion (*Proc. ent. Soc. Wash.* **49** : 155, 157) as follows :—

> "Believing the original Fabrician type of *Crioceris vittata* from Carolina, in the collection of a Mr. Monson, has been lost, I designate as neoholotype and neoparatypes 90 specimens collected on squash, 18th April 1938, at Charleston, S.C., by W. J. Reid, preserved in the United States National Museum. This locality is, most likely, the one from which the original type was obtained. No other Carolinian species in my experience seems to agree so well with the original description. Such confusion has resulted from the brief statement by Lever, 1930, that the type of *Cistela melanocephala* F., 1775, is the species we have called *vittata*, that my action above indicated seems required. If, however, the authentic holotype of *vittata* can be produced and shown to be other than the species here indicated, this action will fall. Lever was not the first reviser.

*The paper here referred to, of which a separate was enclosed with Dr. Barber's letter, is a paper entitled "Science and Legality" published in April 1950 in the *Nautilus* (Vol. **63** (No. 4) : 128-130).

Fabricius himself in 1792 chose *vittata* instead of *melanocephala* as the name for this pest, and page priority claimed by Lever is not here applicable. His choice of the name *vittata* happens to comply with Article 35 of our present Code whereas resurrection of *melanocephala* violates Article 36."

DOCUMENT 2/6

By the late LODOVICO DI CAPORIACCO

(*University of Parma, Italy*)

Extract from an enclosure to a letter dated 31st December 1947

Nomina dubia are undoubtedly an evil for taxonomy, but nevertheless, if neotypes are to be recognised, great care will need to be taken. For, in addition to providing measures to guard against the dangers listed by Mr. Hemming in points (1), (2) and (3) of his paper (1945, *Bull. zool. Nomencl.* **1** : 108-111)*, with which I fully agree, it will be necessary to prevent the acceptance of neotypes from fostering the tendency to consider that type specimens and not descriptions as the base of taxonomy. Such a development would be an evil worse even than *nomina dubia*. It is essential that descriptions should be drawn up in such a way that they are sufficient to permit the recognition of the species described without examining the example (or examples) upon which the description was made, if taxonomists are to be scientists and not merely workers on collections.

In the case of Arachnid species, the designation of neotypes in cases where the type specimens are lost and the descriptions are insufficient for the purposes of recognition, would, I consider, give rise to more trouble than uniformity.

DOCUMENT 2/7

By HORACE H. BAKER

(*University of Pennsylvania, Zoological Laboratory, Philadelphia, Pennsylvania, U.S.A.*)

Extract from a letter dated 22nd July 1950

A " neotype " is a handy standard of reference, but should not be given legal status, because the fundamental principles of the international rules are : (1) The law of priority. (2) Publication. This accent on publication is very important because the printed page never changes, while the care (labels and rearrangements) of museum specimens varies greatly with the individual curator, and, since inflation has embarrassed most museums financially, may become worse instead of better. The Commission should be careful not to discourage careful study in favor of artificiality; e.g., *Opinion* 78 was a ridiculous decision. Such artificiality is the reason why the majority of non-taxonomic zoologists ridicule the International Rules.

The present Commission is doing an excellent job in its suspension of the Rules in certain cases. Such actions are in delightful contrast to the reticence of earlier Commissions in the use of these powers.

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*See Document 2/4.

DOCUMENT 2/8

By G. H. E. HOPKINS, O.B.E., M.A.
(*British Museum (Natural History), Zoological Museum, Tring, Herts, England*)

Letter dated 8th April 1952

I have just been reading, with considerable interest, your article on the neotype question in *Trans. Soc. Brit. Ent.* As Miss Theresa Clay and I have erected quite a lot of neotypes, I think it is up to me to make some comments :—

(i) In Mallophaga neotypes are absolutely essential if we are to attempt to use the older names. Something like 80 per cent. of the types of the species described before 1880 are lost and we are finding more and more instances in which two species which are indistinguishable by means of the old descriptions occur on the same host. Moreover, a large proportion of the older names refer to a complex of several species, and there is no method under the present Rules by which a type host (or locality) can be fixed other than selection of a lectotype ; this cannot be done if the type material is lost, and the only possible way is to erect a neotype.

(ii) I entirely agree with your suggestions (a) that neotypes must be placed in a public museum or other similar institution where they are accessible, (b) should be adequately (what a large question that word begs—and " fully " is worse !) described and illustrated in the paper in which they are proposed, (c) that a paratype should have preference *other things being equal.* But quite often other things may not be equal—it is sometimes possible to state with certainty that a specimen is not from the supposed host, and it would obviously be wrong to make such a specimen a neotype if it can be avoided. Syntypes surely do not come into this question of preference, though you mention them, for if syntypes exist a lectotype can be selected and a neotype is unnecessary.

(iii) Obviously it is desirable that a neotype should also be a topotype, but I am strongly against this provision being made too rigid. Wrong host-records are infinitely more frequent in parasitic insects (Mallophaga in particular) than wrong localities among free-living insects ; you will realize that this is inevitable when a huge proportion of species were described from material obtained either from skins in museums or from captive hosts, in zoos. I would suggest that the neotype should be a topotype unless there is reason to believe that the original locality or host is erroneous, but even this should not be mandatory, because (as you say) the condition might make erection of a neotype impossible. In any case, what *exactly* is a topotype ? Often nowadays an author gives the type-locality almost to a yard—a specimen caught at the exact spot is obviously a topotype, but what of one caught 100 yards away, or 400 yards, half a mile, 2 miles, 10 miles ... ? And parasites very rarely pay any heed to the subspecies of their hosts, but occasionally they do.

(iv) I am most strongly of the opinion that authors (even Miss Clay and myself!) should not be allowed the final word in this matter. One author (I am sure you will guess his name!) has already made one neotype which is not congeneric with the original material; obviously he was misled by an erroneous bit of nineteenth century synonymy and never looked at the drawing published as part of the original description, which is poor but recognizable. I suggest that authors should erect provisional neotypes and that these (after receiving the same sort of publicity as is given to proposals to suspend the Rules) should be confirmed (or disallowed) by the Commission; during the interval they would have provisional validity. Applications to the Commission for confirmation of provisional neotypes should not be confined to the author who erects them.

(v) It is, I think, absolutely essential that, once the Commission has recognized a neotype, that fact should entirely and permanently deprive the original type-material of any status.

(vi) The problem of whether neotypes should go to the museum which once possessed the original material is complicated. Miss Clay and I, for instance, have already transgressed badly against this suggestion. The reason is quite simple: the majority of the species we have dealt with either never were in any museum as far as is known or were at Halle. Now Halle is behind the curtain, and to send specimens there would mean that they would be utterly inaccessible to any western student of the lice. Moreoever, is it very useful to have the types of a few species in some museum where there is only a tiny collection of the group and nobody working on it? This would often be the case.

DOCUMENT 2/9

By L. B. HOLTHUIS

(*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*)

Letter dated 15th April 1952

Neotypes

Your paper (1952, *Bull. zool. Nomencl.* 7 (5/6) : 131-147) has been read with great interest. I can assure you that I am greatly in favour of the recognition of neotypes, of course under adequate safeguards against abuse of them. Perhaps I may make the following remarks :—

(1) Your paragraph **10**, p. 136. It has always been a nuisance to zoologists who had to examine type specimens, that such types generally are scattered over a large number of museums. In this way much time is lost which could have been spent more profitably in the examination of material. Even a concentration of type specimens in a restricted number of institutions is impossible, not the least because of institutional pride ; the ideal of one type museum, apart from severe practical disadvantages, is of course unattainable. With neotypes the Commission indeed possesses the means to restrict the number of institutions in which the types may be stored. I would advocate that the utmost use should be made of this power by limiting the number of Museums in which neotypes may be stored to the minimum. Would it not be possible to make it a general principle that each country should have only one neotype-museum, and that this principle should be abandoned only in very special cases ? This would greatly serve visiting foreign scientists.

(2) Your paragraph **24**, p. 145. It seems best to me to use the name " unofficial neotypes " for those neotypes established before the official scheme concerning neotypes comes into action. The word " neotypes " or " official neotypes " may be used for the other neotypes. Unofficial neotypes should have no standing, I think, and scientists having established unofficial neotypes or being aware of the existence of such unofficial neotypes in their speciality should be urged to take as soon as possible the necessary steps to let these unofficial neotypes become official neotypes.

DOCUMENT 2/10

VIEWS OF SEVEN WORKERS IN SYSTEMATIC ENTOMOLOGY IN THE MUSEUM OF COMPARATIVE ZOOLOGY AND THE BIOLOGICAL LABORATORIES, HARVARD UNIVERSITY, CAMBRIDGE, MASSACHUSETTS, U.S.A.

Enclosure to a letter dated 28th April 1952, from Dr. JOS. BEQUAERT

In answer to your appeal for advice " on the question whether ' neotypes ' should be recognised in the *Règles* as a category of type specimen " : Z.N.(S.) 358 :—

(1) The undersigned are opposed to the insertion in the *Règles* of any provision making it compulsory to observe neotypes as a category of type specimen.

(2) It is felt that the chances for both conscious and unconscious abuse of a compulsory neotype category are so great as to render legitimisation of neotypes under the *Règles* very dangerous to systematics.

(3) We approve insertion in the *Règles* of a provision expressly denying the right of any worker to designate a neotype, binding upon other workers as a typical specimen.

JOS. BEQUAERT
P. J. DARLINGTON, Jr.
E. O. WILSON
PHILLIP A. ADAMS
F. M. CARPENTER
WILLIAM L. BROWN, Jr.
F. Y. CHENG

Workers in systematic entomology in the Museum of Comparative Zoology and the Biological Laboratories, Harvard University.

DOCUMENT 2/11

By D. K. McE. KEVAN

(*School of Agriculture, Zoology Section, University of Nottingham, Sutton Bonington, Loughborough, England*)

(1) Enclosure to a letter dated 29th April 1952

Comments on the problem of neotypes discussed in Part 5 of Volume 7 of the *Bulletin of Zoological Nomenclature*

I am fully in favour of the recognition of neotypes providing sufficient safeguards to prevent their abuse be provided.

Para. **11**

In cases where an adequate description and/or figures exist already for type specimens which have been lost, I am of the opinion that a statement to the effect that a proposed neotype agrees fully with the description and/or illustration of the original type should be deemed equivalent to a description and figure of the neotype. In cases where only minute details differ, I consider that it should be sufficient to point these out. I agree that a complete re-description and re-illustration should be recommended, but I do not think it should be a rule. It might be that the original description or figures are inadequate, but it is equally likely that re-description and re-illustration would be redundant.

Two difficulties arise. Firstly it would be difficult to pre-judge what should be considered an adequate description or illustration and secondly, if re-illustration were to be ruled necessary for the recognition of a neotype, what would constitute an illustration ? Would it in the latter case have to be stated that it was indeed a figure of the neotype (or part of it) itself or would it be sufficient to illustrate the species to which the neotype belonged when designating the neotype. Would a figure need to show the whole animal, or, if only a part, how would one qualify the type of illustration acceptable ? Clearly one could not insist on the whole animal since this is sometimes not known (especially in fossils), but an illustration of a single scale or a bunch of bristles might be quite inadequate by itself although satisfactory if taken in conjunction with previously published figures of the original type or other material.

Para. **15**

I favour alternative (*b*) since a rediscovered holotype or syntype may be clearly shown to belong to a species other than that to which the neotype belongs.

Para. **16**

To *Bull. zool. Nomencl.* could we not add " or some other specified publications " ? I feel that there would be reluctance on the part of many authors, having designated a neotype in one journal, to write a further paper, perhaps stating his reasons again, for the *Bulletin*.

Para. **24**

I suggest that all unofficial neotypes designated prior to any rule coming into effect should automatically be recognised, providing holotypes and syntypes

shown to belong to a different species from the neotype in question are not known to be in existence. If any specialist wishes to have the recognition of any neotype rejected for any other reason than that holotypes and/or syntypes are discovered and shown to belong to a different species, he should make application to the Commission for the purpose.

It should be recommended that all established unofficial neotypes should be brought to the notice of the Commission for consideration and placing upon an *Official List*, but failure to bring forward for recognition any neotype designated before the rules governing neotype designation should not render such designations invalid.

I suggest also that to distinguish unofficial neotypes from official ones, no irritating serial number is required, but merely the date (in brackets) when the unofficial neotype was designated. An official neotype would require no date and an unofficial neotype admitted to the list would automatically be excused from carrying a date thereafter. Unofficial neotypes designated subsequent to the rules governing neotypes would be invalid. Only one neotype should be permitted. An official neotype would take precedence over any unofficial one.

In addition to the above notes I should like to suggest that it should be possible to designate neotypes for species which have been described or figured but not named until a later date (by another author). In such cases there never has been a type specimen, e.g. the Acridid, *Tenuitarsus angustus* (Blanch.) was figured by Savigny (1825 ?) but was not given a name until Blanchard did so in 1836, calling it *Ommexecha angustum*. This species thus never had a type—even if the specimen figured by Savigny were in existence (which it is not). Something similar may be found to be the case with animals invalidly described (either before or after 1758) and later given a valid name. The material used by the original describer cannot be considered the type of the valid species unless it was actually seen* by the author who first gave it its valid name. The "holotype" would technically be the earlier invalid *description* or *illustration* (as with Savigny above) and not the material upon which that description or illustration was based. Where the author who first gave the valid name had not seen the material, it would be necessary to designate as neotype a specimen from among that material if any were found to exist or, failing that, from another reliable source.

If, for example, the original specimen illustrated by Savigny (above) were discovered, it would not be the type of *Ommexecha angustum* Blanch. because Blanchard never saw it. It could, however, be designated a neotype. It might also be placed in a special category taking precedence over any other unofficial neotype (and even perhaps by application to the Commission over any other official neotype ?).

In the case of an invalidly named or unnamed species being subsequently given a valid name by its *original author*, the original material would be deemed typical whether or not it was still available to the author at the time he gave the valid name, unless the author chose to designate other material as typical.

*This might be difficult to confirm or deny. It should therefore only be accepted that he had seen it if it is clear that he had done so—preferably by a statement to this effect.

(2) Extract from a letter dated 12th June 1952

My suggestion on the subject of the publication of proposals relating to the establishment of neotypes in particular cases was due to a feeling that authors wishing to designate neotypes may wish to do so in connection with some taxonomic work which would not be of concern to the Commission, since the latter deals with nomenclature only. Having proposed that such and such a specimen be regarded as a neotype, an author might feel reluctant to write up again the whole of his evidence for regarding it as such. I would agree that formal application to the Commission should be published in the *Bulletin*, but I feel that this should be brief, merely referring to the relevant literature and summarising the case. I feel that the whole case need not be stated in detail, otherwise the *Bulletin* would be in danger of becoming a journal devoted to a certain type of taxonomic problem as much as one of nomenclature.

DOCUMENT 2/12

Suggestions furnished by FRANZ DANIEL

(*Zoologische Sammlung, des Bayerischen Staates, München, Germany*)

Editorial Note.—Attention is drawn to the letter, dated 30th April 1952, from Dr. Franz Daniel (*Zoologische Sammlung des Bayerischen Staates, Müinchen*), which, being mainly concerned with the problem of promoting stability in zoological nomenclature has been included in the series of papers which has been assembled in regard to that subject. It is there included as Document 1/25.

In the letter referred to above, Dr. Daniel suggested that the stabilisation of nomenclature should be sought by the establishment of committees of interested specialists in particular groups at the family level, wide powers being given to these Committees to promote stability without being unduly trammelled by the principle of priority. It will be seen that Dr. Daniel included in his scheme a proposal that power should be granted to the Committees to establish neotypes in cases where by reason of their age or for other reasons the existing type material was insufficient effectively to serve the required purpose.

DOCUMENT 2/13

By CYRIL F. DOS PASSOS, LL.B.
(*Research Associate, American Museum of Natural History, New York*)

Enclosure to a letter dated 3rd May 1952

ON THE QUESTION WHETHER AND SUBJECT TO WHAT CONDITIONS THE CONCEPT OF A " NEOTYPE " SHOULD BE OFFICIALLY RECOGNISED BY AN APPROPRIATE AMENDMENT TO THE " RÈGLES "

Introduction

In recent years it has become the practice among some zoologists to designate neotypes when the type has been lost or destroyed. Examples of this custom among entomologists will be found in the papers of Freeman (1952), dos Passos (1943, 1949), dos Passos and Grey (1947), Sabrosky (1950), and others.

The practice of designating neotypes has no basis at present in the *Règles*, but it grew up, like the practice of designating lectotypes, as the result of what was deemed to be a matter of necessity. The problem presented by the question of recognising neotypes was brought first to the attention of the International Commission on Zoological Nomenclature by Frizzell and Wheeler (1935, 1945) who in the case discussed by them concluded that such recognition would certainly tend to stabilise nomenclature and prevent some of the shuffling about of names, which is so common at present. This interesting paper (1945) was commented upon by the Secretary of the Commission in the same volume (1945) and he brought that question up at the 1948 International Congress of Zoology at Paris, where it was recognised that there is a widespread desire for the recognition of neotypes under certain conditions. At that Congress the Commission agreed to recommend (1950, *Bull. zool. Nomencl.*, vol. 4, pp. 191-193) :—

> " that the Secretary to the Commission should be invited to make a thorough study, in conjunction with interested specialists, of the problems involved in the proposal that the category ' neotype ' should be recognised in the *Règles* and to submit a Report thereon, with recommendations, for consideration by the Commission at their meeting to be held during the next (XIVth) meeting of the Congress, with a view to the submission by the Commission of a considered statement of their views on this subject and, if they decided in favour of recommending that the foregoing category of type specimen should be recognised in the *Règles*, of a comprehensive scheme to that end."

The matter was again discussed informally in the Section on Nomenclature at the 1951 International Congress of Entomology at Amsterdam, where it was described as one of the important matters coming up for decision in 1953. Recently the Secretary of the International Commission on Zoological Nomenclature published an elaborate report on the subject of recognising neotypes with numerous recommendations, and an appeal to zoologists for advice

(1952 *a*). It is to assist in the decision whether (1) neotypes should be recognised by the *Règles*, and if so (2) to formulate appropriate rules covering this subject that the present paper is submitted to the Commission. Some of the Secretary's recommendations will be commented upon at the conclusion of this paper.

Should Neotypes be Recognised?

In zoology a name is applied to an object called the type specimen. The type specimen is identified by a name and defined by a description in words or by a figure, as prescribed by the *Règles*. Words can never fully describe an object because (*a*) they are inadequate, and (*b*) the author may overlook some essential character, etc. Figures also are fallible, although often better than words, but they cannot be dissected—sometimes a matter of supreme importance. Old descriptions are especially troublesome with scanty wording frequently applicable to two or more objects, and with many indefinite or false localities. In every case nothing is better than, and nothing is quite as good as the type specimen itself. If the type specimen is lost or destroyed, the next best thing is a new type specimen—a neotype—believed to be similar to the old one from the same locality, and taking its place.

A neotype is defined as " . . . a specimen identified with a species already described, and selected as a standard of reference where the original type or co-types are lost or destroyed " (Smith, 1906, p. 87), " . . . a later selected type of a species necessitated by loss of the original material; the neotype must come from the original locality " (Frizzell, 1933, p. 658), or more modernly as " . . . a plesiotype . . .; selected to represent the holotype when the original type or holotype is lost or destroyed " (de la Torre-Bueno, 1937, pp. 180, 214). Other definitions have been proposed by Banks and Caudell (1912), Frizzell (1933), and Schenk and McMasters (1935).

There are two additional situations not covered by present definitions in which the designation of a neotype may be necessary or advisable. If a type specimen is unrecognisable as a result of a serious injury, a new type specimen should be designated. An unrecognisable type specimen is no better than one that has been lost or destroyed. Often in Lepidoptera the abdomen of a type specimen is missing. Many species of Lepidoptera can be determined correctly only by an examination of their genitalia. This is an illustration of a case where the type specimen, although in existence, is not recognisable. The second case is where a type specimen is fragmentary. Perhaps a name may have been proposed from a bone or a tooth, and later a complete skeleton is discovered. In such an instance a paleontologist may well wish to designate a neotype for the complete animal. Certainly that would be highly desirable. Such cases should be provided for if neotypes are to be recognised by the *Règles*.

The principal objection to the designation of neotypes is that it would encourage unscrupulous dealers and others to create such types for commercial or private purposes. This objection is met by a provision in the proposed rules, hereinafter set forth, that neotypes shall not be created *en masse* and must be deposited in certain specified institutions. Thus the incentive to designate neotypes for personal gain is removed. There are also those who believe that neotypes are unnecessary. That obviously is a matter for each

individual worker to decide for himself. Perhaps in some cases they are unnecessary to some individuals, but to others the contrary is often true. Others have inquired, " What becomes of the neotype if the type is rediscovered at some later time ? " There appears to be no objection to having the neotype fall if the type is subsequently discovered. No new name has been created. If the neotype is found not to be conspecific with the type, a new name could then be proposed for it if necessary, and all references thereto placed in the synonymy of the new name. This phase of the matter seems to present no complications. On the whole it is believed that neotypes under some terms and conditions should be recognised by the *Règles*.

What Conditions Should be Required by the " Règles " to Authorise the Designation of Neotypes ?

With a view to advancing the discussion of the subject so that rules may be drafted and circulated among zoologists before any action is taken on this important subject by the International Congress of Zoology at Copenhagen in 1953, the following rules for the regulation of neotypes are suggested :—

DEFINITION. A neotype is a specimen designated to replace the holotype or the lectotype (both hereinafter referred to as the type specimen), when the type is lost, destroyed, unrecognisable, or fragmentary. A type specimen is unrecognisable or fragmentary when it lacks a character necessary or helpful in establishing its correct determination.

RULES. After —th January 195[?], an author may designate a neotype by complying with the following rules :—

A. The type must be lost, destroyed, unrecognisable, or fragmentary, and detailed evidence of that fact must be set forth in the paper containing the designation.

B. The designation of the neotype must be (1) published, (2) the neotype must be labelled as such by its designator, and (3) the neotype must be deposited in a recognised scientific or educational institution which maintains a research zoological collection with proper facilities for conserving types and giving access to its collection by accredited students.

C. The neotype must agree with the original description and any published redescription of the type before its loss, destruction or unrecognisableness, or with the fragment described in an original description.

D. The neotype must agree also with the first published restriction of the species, even though not accompanied by a lectotype or neotype designation.

E. The neotype must be (1) from approximately the same locality, if definite, as the type specimen, or if indefinite, from a locality subsequently fixed, (2) taken at approximately the same season, and (3) of the same sex and host as the type, if any or all these facts are known.

F. If the type locality, as originally published, is shown to be false, the neotype must be from approximately the same locality as that subsequently fixed.

G. Neotypes shall not be designated *en masse*, but (except as hereinafter provided) in a revisionary paper only, to which the designation is relevant and material.

SAVING CLAUSES. Nothing herein contained shall be deemed to :—

1. Invalidate any neotype designation published on or before 31st December, 195[?], in conformity with these rules, and the burden of proving non-conformity shall be upon the author alleging that fact, but such neotype, if published in

substantial conformity with these rules, may be republished in conformity therewith in a paper whether revisionary or not, and shall date from the time of the original publication.

2. Validate any neotype designation whenever published, if at any subsequent time the type is found, unless it is unrecognisable or fragmentary.

PENALTY. After —th January 195[?], a neotype not designated in accordance with these rules shall be null and void, and shall not prevent the subsequent designation of a neotype in accordance with the provisions hereof, but in such event the author shall set forth in detail the reason for claiming that the prior designation is invalid.

Comments on the Secretary's Recommendations

Careful consideration has been given to the proposals of the Secretary (1952 *a*) with many of which agreement will be almost unanimous. There are others, however, which seem unnecessary, or unduly restrictive, such as those wherein he recommends, among other things, the institution of some central authority, without the approval of which no neotype could be validly established (paragraph 8), or by which the neotype would be designated (paragraphs 9 and 20), and by which the depository would be selected in which the neotype would be placed (paragraph 10), and they are not favoured. The same reasoning applies to the publication of a figure (paragraph 11) of the neotype—something not required for a type or lectotype, and the suggestion also advocated by Usinger (1952) that a new description be published (paragraph 11), and that these be overseen by the central authority. Who, it may be asked, is to pay for such a figure in a day when the cost of illustrating a paper has become almost prohibitive, and why is a new description to be required when in most cases there already is a description of the type ? May there not be danger that two descriptions will conflict with each other in some particular ? With the Commission lacking as it does both a sufficient staff and ample funds, there is no central authority in existence capable of handling all the work envisaged by the Secretary, and most of it is no more necessary in the case of a neotype than in the case of a holotype or a lectotype. Neither the Commission nor its Secretary should be burdened with such a vast amount of additional labour, and how, it may be inquired, is the financing of a central authority to be provided for ?

Neotypes should be considered no more sacred than holotypes or lectotypes, except insofar as it is necessary to prevent abuse in designating them. Until some provision is made for the care and accessibility of holotypes and lectotypes, no provision on these subjects need be made for neotypes other than requiring their deposit in a suitable institution, something that should be compulsory for all types. In the meantime it may safely be left to the author to decide where neotypes will be deposited, trusting to his interest to see that they are preserved and made accessible to students, as is the case at present with other types.

It is believed that the Secretary's well-intentioned desire to secure adequate safeguards against the exploitation of neotypes for commercial reasons or otherwise, with which everyone must be in accord, has moved him to suggest such drastic requirements in some cases that if adopted the desirable authority to designate neotypes will be frustrated in many instances and that few will

be created. The freedom and responsibility of authors must be maintained. Let us by all means have neotypes, but let them be designated under simple, self-operative rules which each author may apply on his own responsibility.

In conclusion, I oppose the suggestions appearing in paragraphs 8 to 11 of the Secretary's report (1952 *a*), including the so-called need for the avoidance of duplicate neotypes (paragraph 9)—something easily controlled by the law of priority as it is in the designation of types. A second neotype, erroneously designated, would merely become a synonym of the first. I disagree also with paragraph 15, depriving of its status a subsequently discovered type believed to have been lost or destroyed, unless it is found to be unrecognisable or fragmentary, and with paragraph 16, requiring the fullest discussion between interested specialists, as often impracticable and leading to much waste of time and no tangible results when such specialists disagree, and paragraph 17, requiring approval by a central authority for neotypes.

As to the type locality from which the neotype must be selected, that involves other considerations discussed by the Secretary in a separate paper (1952 *b*, pp. 172-180), and may perhaps not necessarily be gone into any further in the present paper. It suffices to say that the neotype must be from the type locality, subject, however, to certain obvious exceptions, which have been provided for in the rules herein proposed.

Acknowledgment

I am indebted to Professor Alexander Barrett Klots, of the American Museum of Natural History and the College of the City of New York, and Mr. Curtis Williams Sabrosky, of the Division of Insect Detection and Identification, United States Department of Agriculture, Bureau of Entomology and Plant Quarantine, Washington, D.C., for valuable suggestions in the preparation of the above rules, many of which have been incorporated in words or in substance. Mr. Sabrosky has been particularly obliging in reading several times and commenting upon these suggestions at considerable length. While we have not agreed fully on all of their provisions, it is a pleasure to state that we found ourselves to be of one mind upon many of the questions involved. I have also discussed the proposed rules with Mr. Frederick Martin Brown, of the Fountain Valley School, Colorado Springs, Colorado, who has made valuable suggestions too.

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DOCUMENT 2/14

By Th. HALTENORTH

(*Museum, München, Germany*)

Statement, dated 14th May 1952, communicated by Professor E. M. Hering

VI. Die im "Richter" p. 24-34 gegebene Typen-Handhabung sollte verbindlich gemacht werden, da danach allgemein gearbeitet wird.

DOCUMENT 2/15

By W. J. ARKELL, M.A., D.Sc., F.R.S.

(*Sedgwick Museum, Cambridge University, Cambridge*)

Enclosure to letter dated 23rd May 1952

I am not in favour of the official regulation of neotypes by additions to the Rules. Neither the setting up of neotypes nor their acceptance by later workers has given any trouble in my group. Some people have been motivated by patriotism and have figured a German specimen (say) as neotype of an English species. One simply ignores such performances, and no harm is done. If setting up of neotypes becomes legalised I am afraid some authors who have hitherto been fond of "emending" every species and genus will find ways and means of doing the job with more effect by setting up neotypes wherever possible. Subsequent authors will then have the added burden of sifting the credentials of hundreds of neotypes to ascertain whether they must be accepted under the Rules, with consequent changes of concept.

In recent years some type specimens from the Sowerby Collection have found their way to the B.M. on the death of a relative—over a century after publication. It seems to me intolerable that such types should lose their legal status in favour of a neotype trotted out by some upstart "reviser" in the meantime, on the ground that "the types are lost."

There are so many shades and degrees of adequacy and inadequacy in type specimens that it would be impossible to define at what stage of inadequacy a neotype becomes justifiable. I can see this sort of thing: "Sowerby's specimens are all hopeless. Neotype author's coll. No. 4265783." All this is a further burden on subsequent workers.

There is no reason why an author should not accept a predecessor's neotype if it is reasonable; but why should he be forced to do so if he is convinced that it is not the same as the original types or type figures, which he may know better than the less conscientious predecessor?

DOCUMENT 2/16

By J. R. DYMOND
(*University of Toronto, Department of Zoology, Toronto, Canada*)

Statement received on 27th May 1952

I am in favour of the recognition in the *Règles* of the concept of neotypes as a category of type specimen and I consider the draft plan submitted in Volume 7, Parts 5 and 6, of the *Bulletin of Zoological Nomenclature* adequate for the decision of question arising in connection with the designation of neotypes.

DOCUMENT 2/17

By ERICH M. HERING
(*Abteilungsleiter am Zoologischen Museum der Humboldt-Universität zu Berlin*)

Statement received on 7th June 1952

Stellungnahme zur vorgeschlagenen Einführung des " Neotypus "-Begriffes in die " Règles "

Die Vorschläge über die Einführung des Neotypus in die *Règles*, die in *Bull. zool. Nomencl.* **7,** Pt. 5/6, p. 133-147 ausgeführt wurden, werden vom Stab der Zoologen des Zoologischen Museums Berlin mit 13 von 14 Stimmen, vom Stab des Zoologischen Museum München (nach Mitteilung von Dr. Walter Forster von der Zoologischen Sammlung des Bayerischen Saates) mit 11 von 12 der befragten Zoologen *unterstützt.*

Die in Para. **7** (p. 134) vorgesehenen Sicherungs-Klauseln werden von den Zoologen beider Museen als unbedingt notwendig angesehen.

Die Paras. **8-14** (p. 135-138) werden mit 13 von 14 Stimmen des Museums Berlin und mit 11 von 12 Stimmen des Museums München *unterstützt.*

Von Para. **15** wird der Vorschlag (*a*) (p. 140) mit 13 von 14 Stimmen des Museums Berlin *abgelehnt.* Es soll also der Neotypus seinen Status verlieren, wenn Holotypus oder Syntypen wiedergefunden werden und sich als zu einer anderen taxonomischen Einheit gehörend erweisen.

Aus Para. **13** (p. 137) werden die Worte " in such a case, it may be thought desirable to designate as the neotype a more representative example " mit 13 von 14 Stimmen des Zoologischen Museums Berlin *abgelehnt.*

Zu Para. **24.** Unoffizielle Neotypen, die bisher aufgestellt worden sind, werden von den Zoologen des Berliner Museums mit 14 von 14 Stimmen *abgelehnt*. Dr. Walter Forster fordert im Einverständnis mit dem Zoologen-Stab des Münchener Museums, dass solche Neotypen durch ein entsprechendes Verfahren *legalisiert* werden sollen.

DOCUMENT 2/18

By RICHARD MEINERTZHAGEN, D.S.O.
(*London*)

Extract from a letter dated 16th June 1952

I have just read your *Bull. zool. Nomencl.* 1952 paper on neotypes.

I agree with all your safeguards, but it may be very difficult for a central authority to assess the efficiency of museums to look after their types. I think it might be better if agreement could be reached by which each country selected one of its museums for reception of neotypes ; for example, in this country the B.M.

I do not like the idea of inventing a new name for agreed neotypes. It would overburden an already congested type-nomenclature.

I should like to insist that all descriptions of neotypes be in English, French or German.

DOCUMENT 2/19

By ANGEL CABRERA
(*Eva Peron, F.C.N.G.R., Argentina*)

Statement dated 22nd June 1952

On Neotypes. Z.N.(S.)358

I am in favour of the recognition in the *Règles* of the concept of neotype, and I agree with your suggestions in paragraphs **21-24** (pp. 141-146). It seems to me, however, that it is very important to distinguish between the two kinds of neotypes : (1) neotypes s.s., to be designated because the type material was lost, or supposedly lost, and (2) complementary neotypes, designated because the type material is considered either insufficient or inadequate for exact specific identification, as in the case of a partly destroyed specimen, or a fossil type consisting only of isolated teeth. Now, a difficult question is the one exposed in your paragraph 15. What must we do if, after designating a neotype, the supposedly lost type (case 1) reappears, or the parts missing in the defective type (case 2) are found ? You suggest (p. 140) two alternatives : (*a*) to deprive all the surviving type material of its status as such, and (*b*) to rely on a decision of the International Commission. I am sorry that I

dissent from your suggestion that " of these alternatives (*a*) is preferable to (*b*)." In fact, alternative (*a*) means to deprive an author of the paternity of his species. And what if, through comparison between the neotype and the rediscovered (or completed) original type, they are found to belong to different species ? E.g. John Smith describes a new species, names it and designates a type. Afterwards, by war, fire or earthquake, the type appears to be lost, and Peter Jones designates a neotype, subsequently recognised as such by the International Commission. But five, six or twenty years afterwards the supposedly lost type is found, and careful comparison reveals that the neotype belongs to another species. Is it fair to assert that the neotype designated by Jones represents the " final and irrevocable standard " for identifying a species to which it really does not belong ? How is it possible to say that the characters of the species to which the neotype belongs are those of the species described by Smith, thus ascribing to Smith the authorship of a species which he did not even know ? We cannot build a system of zoological nomenclature on false foundations.

As to incomplete types, so long as the *Règles* permit a nominal species to be described and established on " any part of an animal," I cannot see how we can deprive a type of its status as such on the ground that it is a fragmentary specimen.

In my opinion, a neotype s.s. ceases to be such if the original type reappears, and a complementary neotype retains this status only as long as it is not shown to represent a species different from the species of the original type.

DOCUMENT 2/20

By JOSHUA L. BAILY, Jr.
(*San Diego, California, U.S.A.*)

Statement received on 24th June 1952

Neotypes (Bull. zool. Nomencl. 7 : 131-147)

Para. **8**

The fact that the original type specimen of a species was placed in a certain museum is no reason, I think, for ruling that in the event of their destruction or loss, the eligible species to be selected as neotypes should be confined to specimens in the same museum. The type specimen should be kept in a museum near the type locality.

As an example I might mention the family ACHATINELLIDAE, which is confined to the Hawaiian archipelago in the mid-Pacific. The types of many of its species are in Vienna. The inconvenience that confronts the student who has to go to Hawaii to collect his living material and to Vienna to study the types is obvious. If as the result of any calamity the types in Vienna should be destroyed the neotypes should be selected from the museum in Honolulu.

DOCUMENT 2/21

Statement of the views of the scientific staff of the
ROYAL ONTARIO MUSEUM OF NATURAL HISTORY, TORONTO, CANADA

Enclosure to a letter, dated 26th June 1952, from Dr. F. A. URQUHART
Director

(For the text of the above letter, see Document 1/39 (pp.67-68)

Neotypes : Commission's Reference Z.N.(S.)358

We are unanimously in agreement that the concept of neotypes should be recognised in the rules as a category of type specimens.

We unanimously agreed that provision should be inserted in the rules stipulating that neotypes must become the property of a museum or other public institution or, on being so selected, be presented to, or placed on permanent loan in such an institution.

We unanimously agreed that a full description and figures of specimens should be presented when designating a neotype. Further, that a specimen to be a neotype should conform as closely as possible with the original description of the species concerned.

We unanimously agreed that a neotype must not necessarily be a specimen from the same locality as the original type material of the species concerned.

It was unanimously agreed that, on the establishment of a neotype, all surviving type material, whether at that time known to be in existence (this being a provision which, as already shown, would in any case be necessary for other reasons) or discovered subsequent to the establishment of a neotype for the species in question, should be deprived of its status as such.

It was unanimously agreed that, before any specimen is officially recognised as a neotype, there should be the fullest consultation between interested specialists.

Suggestion

The suggestion is offered that central bodies be set up in connection with establishing neotypes. These bodies would be connected with institutions concerned with the systematic studies of a particular Order. For example, at the present time Dr. Rehn of the Philadelphia Academy of Sciences might be elected to oversee the erection of neotypes in the Order Orthoptera in North America. Individuals for the study of other Orders of animals might be similarly elected to pass judgment on the establishment of neotypes, and also through consultation with other workers in the Order, to decide what institution should receive the neotype material. If such is deemed necessary, a public notice might be published before approving the establishment of a neotype. It is our opinion, however, that acting through the office of an elected person outstanding in research in a particular Order, correspondence would eventually solve the question without publication. We feel that such a suggestion would take care of most of the questions presented by you in

paragraph 20, page 143, of Volume 7, Parts 5 and 6. We might further suggest that such elected individuals might work through the International Commission on Zoological Nomenclature, if such is deemed advisable. It might well be that this particular individual (or committee) in charge of a particular Order might make application to the Commission for permission to designate a neotype, after having duly considered the matter. It is further proposed that the International Commission would prescribe rules regarding the information to be furnished in any application for the designation of neotypes to that person or persons so designated as in charge of the systematic study of a particular Order.

DOCUMENT 2/22

By A. MYRA KEEN and SIEMON W. MULLER
(*Stanford University, Stanford, California, U.S.A.*)

Enclosure to a letter dated 1st July 1952

On the Question of Recognition of " Neotypes " in the " Règles "

We favour the acceptance of " neotypes " as a category of type specimen with the safeguards suggested—namely, that the International Commission on Zoological Nomenclature have exclusive right to designate the type specimens and that such types should be registered on the *Official List of Specific Trivial Names.*

DOCUMENT 2/23

Statement of the views of the
COMMITTEE ON NOMENCLATURE OF THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Enclosure to a letter dated 10th July 1952

(For an extract from the above letter, see Document 1/41 (pp. 70-71)

General designation of neotypes should be placed under high restrictions. It is questionable whether they should be permitted at all, since they may easily serve to fix a name on a totally different concept than that envisaged by the original author. If the name cannot be identified without the (lost or deficient) holotype, how can its exact identity be bonded with a specimen arbitrarily selected by anyone, including the original author ? If the name is identifiable, no neotype is needed. Unidentifiable names are provided for by the class of *nomina dubia*, where they should remain. Yet, where names are, strictly speaking, unidentifiable, but are of universal usage to designate well-known species, such usage is to be maintained.

However, we realise that neotypes are in favour among certain taxonomists, particularly palaeonotologists, who find them of particular service as adjuncts to deficient holotypes rather than as replacements for lost holotypes. If they are recognised by the Commission, restrictions should require documented and adequate proof of the absolute necessity for each such neotype; the designation should be made only in a complete review of the group concerned; it should be approved by a committee of experts in its limited field and after advance notice through the Commission; the neotype should be deposited in a recognised public museum of international repute.

One question involves the procedure in case a supposedly lost holotype is rediscovered. Mr. Hemming proposes two alternate solutions. (1) The holotype will have lost all rights; (2) The case must be re-examined by the Commission. We favour alternate No. 2. Any automatic ruling could easily upset existing nomenclature. The first alternative emphasises the possibility that the neotype may prove to be something other than the holotype, showing that what is being done is creating a new concept and attempting to use an old name for it which is of uncertain application.

A.M.N.H. Committee on Nomenclature.

(Signed) JOHN T. NICHOLS.
ERNST MAYR.
GEORGE H. H. TATE.
JOHN T. ZIMMER (Chairman).

Edwin H. Colbert looks with more favour on the use of neotypes, but believes with us that they should be used sparingly and subject to careful control. If lost types are found, he agrees the matter should be referred to the Commission.

(Signed) JOHN T. ZIMMER.

DOCUMENT 2/24

Statement submitted on behalf of the
NOMENCLATURE DISCUSSION GROUP, WASHINGTON, D.C.

Enclosure to a letter, dated 16th July 1952, from
Dr. R. E. BLACKWELDER, Secretary

Neotypes

Discussion of this problem revealed such wide and strong differences of opinion that it appeared impossible to draft any general statement for the Nomenclature Discussion Group. Accordingly, a ballot was prepared, containing brief statements of various important points, and the results of the vote are herewith submitted to the I.C.Z.N.

(1) **18.** Neotypes should not be recognised in the Code.

31. Neotypes should be recognised in the Code.

All but three of those opposed to neotypes also registered their beliefs on the following questions that would have to be considered if neotypes were adopted. A breakdown of the votes showed approximately the same reactions to the other questions on the part of those against neotypes and those for neotypes, except in the fourth question.

On most points there was a definite majority in favour of one view, but with a sizeable minority. On the sixth and seventh points, however, the margin was particularly noteworthy.

(2) **20.** They should be governed by definite rules in the Code.

26. The Code should contain a few basic rules, with recommendations for matters where mandatory provisions seem impractical (e.g. agreement with type locality).

(3) **11.** Neotype designation should be vested exclusively in the International Commission.

35. Designation should be by individuals, operating under rules and recommendations in the Code.

(4) **30.** Neotypes should replace only lost or destroyed types.

16. Neotypes should replace fragmentary or unrecognisable types as well as those lost or destroyed.

On this point, the taxonomists who are fundamentally opposed to neotypes voted 12 to 3 for the first alternative (neotypes to replace only lost or destroyed types). Among those favourable to neotypes, however, the vote was much closer, with the first alternative favoured by 18 to 13.

(5) **25.** Neotypes should be deposited only in museums, or other public institutions suitable as type depositories.

10. As above, but if the original type was in a certain museum or comparable public institution, or in a collection now the property of such, the neotype must be deposited there, unless such place no longer maintains type or research collections.

9. Neotypes may be deposited in any collection, public or private, as the designator may choose.

2. [For the original collection, whether public or private, or if destroyed, then in any collection.]

It is possible to analyse these answers in various ways. Thirty-five favour putting neotypes in museums or other suitable public institutions, as against

eleven who favour the right to put them in any collection, public or private. Twelve would give precedence to the original collection, but thirty-four would not insist on that rule.

(6) **40.** Neotypes automatically fall if types are later found.

6. Neotypes, once established, take precedence over any type material subsequently discovered.

(7) **40.** Neotypes should be designated ONLY where relevant and essential to solving a zoological problem (ideally in revisions and monographs as those terms are usually understood, but acceptable under the above limitation in a paper dealing with the status of one species or its subspecies).

6. There should be no restraints on when neotypes may be designated

DOCUMENT 2/25

By HENNING LEMCHE
(*Universitetes Zoologiska Museum, Copenhagen*)

Extract from a letter dated 20th July 1952

On your interesting and exhaustive comment [in Vol. 7 of the *Bull. zool. Nomencl.*] on neotypes (Z.N.(S.)358) I have no comments.

DOCUMENT 2/26

Statement submitted on behalf of the
NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY

Letter, dated 21st July 1952, with enclosures, from
Dr. W. I. FOLLETT, Chairman

The Nomenclature Committee of the Society of Systematic Zoology recommends as follows :—

(1) The Rules should recognise the concept of neotypes.

(2) Neotypes should be confined to the replacement of lost types.

(3) Neotypes should be designated by individuals, not by the International Commission.

On the remaining aspects of this subject concerning which you have requested advice, there is considerable divergence of opinion within the Committee, which is best illustrated by the replies to the two memoranda that were issued to the Committee during the chairmanship of Dr. Charles D. Michener. This material (enclosed herewith in duplicate) comprises the following :—

(*a*) Memorandum to the American Committee on Entomological Nomenclature on the subject of neotypes, by Robert L. Usinger.

(*b*) Memorandum on neotypes, by C. F. Dos Passos (Appendix 2).

(*c*) Reply of each Committee member to the foregoing memoranda. Some of these have been submitted on page 4 of a copy of Dr. Usinger's paper ; others by separate letter (Appendix 3).

Further discussion of this subject by a member of this Committee (and by other members of the Society) is submitted by way of the minutes of a symposium recently conducted by the Pacific Section of the Society (Appendix 4). Only that portion of these minutes which deals with neotypes is enclosed. This material does not constitute a part of the Committee's report, but is submitted by way of a supplement thereto.

APPENDIX 1 TO DR. W. I. FOLLETT'S LETTER OF 21st JULY 1952

MEMORANDUM SUBMITTED BY PROFESSOR ROBERT L. USINGER TO THE AMERICAN COMMITTEE ON ENTOMOLOGICAL NOMENCLATURE

MEMORANDUM TO THE AMERICAN COMMITTEE ON ENTOMOLOGICAL NOMENCLATURE ON THE SUBJECT OF NEOTYPES

By ROBERT L. USINGER
(*University of California*)

I have been asked by Chairman E. G. Linsley to prepare a brief on the question of neotypes for consideration by our committee. This was prompted by the appearance of a preliminary consideration of this subject by the Secretary of the International Commission, Francis Hemming (*Bull. zool. Nomencl.* 7 : 131-147, 1952). Secretary Hemming requests that the views of our own and other committees be sent to him by 31st July 1952, so that they may be considered while preparing the Report which he is to publish in the *Bulletin of Zoological Nomenclature* as a basis for discussion at the Copenhagen Congress.

The procedure that I have chosen for this memorandum is (A) A summary of Secretary Hemming's views as expressed in the *Bulletin of Zoological Nomenclature ;* (B) A critique of these views ; (C) Recommendations on which a vote is requested by our Committee.

A. Summary of Secretary Hemming's Discussion of Neotypes

(1) Purpose : " The sole purpose of authorising the establishment of neotypes is to provide a final and irrevocable standard for the identification of a species in cases where there is no surviving type material or where the known surviving type material is insufficient for this purpose " (B.Z.N. 7 : 140, 1952). Secretary Hemming adds that " The official recognition of a specimen as a neotype amounts in effect to the provision of an entirely new basis for the identification of the species concerned, while retaining for that species its original name with its original author and priority."

(2) No specimen should be eligible for designation as a neotype unless it is, or becomes, the property of a museum.

(3) Museums have a moral claim to the right to designate neotypes for lost types which were once in their possession.

(4) Rigorous safeguards are needed in neotype designation to prevent exploitation by museums and by individuals.

(5) The power to designate neotypes should be vested exclusively in the International Commission.

(6) This central authority is needed to avoid competing or duplicate neotypes, to determine which institutions can be relied upon to take adequate care of neotypes, and to decide when to permit departure from original descriptions and from original type localities.

(7) Neotype designation must be accompanied by a full description and figures.

(8) Neotypes may be designated to supplement original but inadequate type material.

(9) Neotypes take precedence over any surviving type material.

(10) Public notice should be given twelve months in advance of neotype designation.

(11) The neotype should not run counter to any restrictions or restricted identifications and should agree with the appropriate subspecies in cases involving polytypic species.

(12) Neotypes should be substituted for the figures cited by the Commission (*Danaus plexippus* Linn., etc.) as the basis for identification.

(13) The International Commission should prescribe the information to be furnished in applications relating to the designating of neotypes.

(14) Official neotypes should be differentiated by a code number such as " I.C.Z.N./1 " from the unofficial neotypes established before the new scheme comes into operation, the unofficial neotypes to have no status in nomenclature.

(15) The trivial name of every nominal species for which a neotype is established should be entered on the " Official List."

B. Critique of the Above Proposals on Neotypes

(1) The neotype concept, originally confined to the replacement of lost types, has been expanded to cover cases where existing type material is insufficient to provide a standard for the identification of species. Worthy though this might at first appear, it introduces a new subjective element into the picture because there will be differences of opinion as to the adequacy of most type material. The action of the " First Reviser " has sufficed in the past to take care of questions of this kind.

(2) Museums are generally considered to be the rightful depositories for *all* primary types, it being generally agreed that such specimens, upon which scientific work has been based, are the property of science. Nevertheless, this is no more than a recommendation for holotypes, etc., and it is not clear that neotypes are in any way more sacred than holotypes. It would appear that in the present state of international freedom in systematic zoology any central body that sets itself up as an arbiter as to the adequacy, competence and moral claim of competing museums and individuals is doomed to failure.

(3) Exploitation of neotypes could presumably be prevented just as well in the case of individuals as institutions (just as exploitation of the secondary homonym procedure can be prevented) by action of the International Commission. The difference between the present method of designating neotypes (by individuals) and the proposed method (exclusively by the International Commission) is that the individual taxonomist is provided with a procedure that he can use when he needs it, e.g. when he is doing the work. By the other method a *formal* petition must be submitted to the already hopelessly overworked Commission. Under the present "unofficial" scheme the Commission would have to act only in the rare instances when actual abuses have been noted and have been submitted to it for adjudication.

This is the fundamental issue in all recent publications from the Commission. On the one hand statistics are provided to show that the Commission is swamped with more applications, more letters, etc., per year than in all of the previous fifty years combined. On the other hand the trend, as in the present neotype recommendations, is away from so-called "automatic" nomenclature and toward central authority with the inevitable result that more and more cases must be referred to the Commission for arbitrary action.

(4) Central authority is not needed to avoid competing and duplicate neotypes, since priority of designation is a perfectly satisfactory criterion in such cases; no central authority should be charged with keeping the museums of the world under surveillance in order to insure that adequate care is being taken of neotypes. No central authority need decide when to permit departure from original descriptions or original type localities if the Rules were simply to require that neotypes be not inconsistent in characters and type locality with the data provided in the original description.

(5) The requirement that a full description and figures be provided when designating neotypes is fine but is an "ideal" recommendation. Neotypes should be validated by the same requirements as holotypes, e.g. the provisions of Article 25, the Law of Priority.

(6) Public notice could serve a useful function in neotype designation and still not require formal action of the Commission and twelve months' public notice in the *Bulletin of Zoological Nomenclature*. A method might be to allow a twelve months' period, or even longer, *after* the designation of a neotype by a specialist during which period the neotype is open to challenge. If at the end of the specified period no protest has been received by the Commission, the neotype is deemed to have been accepted by taxonomists and is not subject to change without formal action by the Commission.

(7) It should be realised that due to the fragile nature of most scientific specimens, nearly all original types will ultimately have to be replaced, in 100 or 1,000 years. Seen in this perspective, the procedure to be set up should be geared to *regular taxonomic practice* rather than to a central authority.

APPENDIX 2 TO DR. W. I. FOLLETT'S LETTER OF 21st JULY 1952

Notice of a Meeting of the NOMENCLATURE DISCUSSION GROUP, Washington, D.C., U.S.A., called for 18th June 1952 for the purpose of considering the question of the recognition of neotypes as a category of type specimen

NOMENCLATURE DISCUSSION GROUP

U.S. National Museum.
13th June 1952.

NOTICE TO ALL TAXONOMISTS:

The twenty-seventh meeting of the Nomenclature Discussion Group will be held in Room 43 of the U.S.N.M. on Wednesday, 18th June 1952, at 2.00 p.m.

SUBJECT: The proposals on neotypes.

CHAIRMAN: Curtis W. Sabrosky.

PROPOSAL BY C. F. DOS PASSOS on neotypes (in press, *Bull. zool. Nomencl.*).

[EDITORIAL NOTE: At this point there followed in the foregoing Notice the text of the proposals prepared by Mr. Cyril D. Dos Passos which have been published as Document 2/13 in the present Part of the *Bulletin* (pp. 121-127).]

APPENDIX 3 TO DR. W. I. FOLLETT'S LETTER OF 21st JULY 1952

COMMENTS FURNISHED BY THE MEMBERS OF THE NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY ON THE DOCUMENTS CONSTITUTING APPENDICES 1 AND 2 TO DR. FOLLETT'S LETTER OF 21st JULY 1952

Enclosure 1 to Appendix 3 to Dr. Follett's letter

COMMENT FURNISHED BY DR. W. I. FOLLETT

I am opposed to the recognition of neotypes, since I believe that (1) they would produce more confusion than would the absence of type material, (2) they would offer an inducement to the destruction of original type material by misguided workers, and (3) they would add a new subjective element to the Rules.

Enclosure 2 to Appendix 3 to Dr. Follett's letter

COMMENT FURNISHED BY PROFESSOR CHARLES D. MICHENER

I agree with Dos Passos' proposal except that I believe neotypes should be designated only for lost or destroyed types, not for unrecognisable ones. By whom is a type determined to be unrecognisable ?

Enclosure 3 to Appendix 3 to Dr. Follett's letter

COMMENTS FURNISHED BY DR. ROBERT R. MILLER

The Proposals on Neotypes

Views on suggestions by Hemming (*Bull. zool. Nomencl.* **7** : 131-47, 1952) as commented upon by Usinger and on proposals by Dos Passos :—

(1) A neotype should be designated only when the holotype or lectotype is lost or destroyed. To include designation where type material is " insufficient," " unrecognisable," or " fragmentary " introduces a subjective element which I believe to be undesirable.

(2) Neotypes should be governed by the same recommendations used for holotypes and should be deposited in a recognised scientific or educational institution that maintains a research collection (but not necessarily a museum, *sensu stricto*).

(3) Neotypes should be designated by individuals, *not* by the International Commission.

(4) Museums have a moral claim to the right to designate neotypes for lost types once in their possession or, if designated by another worker, museums have a moral claim to the specimen designated.

(5) Rigorous safeguards (see items 4 and 6 in Usinger's summary of Hemming's discussion), etc., are not necessary.

(6) It is strongly recommended (but not required) that neotype designation be accompanied by a full description and figure(s). The designation must be published, and the specimen must be labelled as such by its designator.

(7) The designation of a neotype becomes invalid if at any subsequent time the type (cotype, holotype, lectotype) is found.

(8) Public notice in advance of a neotype designation should not be mandatory. Usinger's suggestion for a period of twelve months (or longer) after the designation during which the neotype selection is to be open to challenge meets with my approval.

(9) The neotype should agree closely (but not necessarily in every detail) with the original description. It should be (*a*) from the type locality (if known) or, if the species is no longer extant there, from a locality nearby that is subsequently fixed, (*b*) taken at approximately the same season, and (*c*) of the same sex and host as the type, if any or all of these facts are known.

Annexe to Enclosure 3 to Appendix 3 to Dr. Follett's letter

COMMENT BY DR. ROBERT R. MILLER IN A LETTER TO DR. FOLLETT

As to neotypes, I agree with the thesis as presented by you over John S. Garth's signature. I strenuously object to the purely subjective clause inserted by Hemming that a neotype can be designated when the known surviving type material is insufficient for identification. I also sympathise with the discussion regarding type locality since there are numerous instances in ichthyology where the species or subspecies is now extinct at the type locality. Above all things, neotypes should not be subjected to the exclusive jurisdiction of the International Commission.

Enclosure 4 to Appendix 3 to Dr. Follett's letter

COMMENTS FURNISHED BY MR. CURTIS W. SABROSKY

(*Editorial Note :* The numbers cited by Mr. Sabrosky refer to the numbers given by Professor Usinger in Sections A and B of the paper which formed Appendix 1 to Dr. Follett's letter of 21st July 1952. Where at the beginning of an item a sentence is placed in inverted commas, this signifies that that sentence forms part of the Voting Paper issued by the Committee to its members.)

Section A

(1) " I am in favour of the recognition in the Rules of the concept of neotypes as a category of type specimen." [Answer given.] Yes, but only if strict regulatory provisions are included.

(3) I agree with numbers 2, 3, 4, 9 (if they are designated in line with strict requirements), 11, 12 and disagree with numbers 5, 6, 7, 8, 10 (but I should like to require prior consultation with other specialists), 13 (certain things should be required by the rules, however), 14, 15 (one might wish to designate a neotype for a synonym) in Section A above.

Section B

(1) Agree.

(2) I favour a rule that both holotypes and neotypes should be deposited in a reputable institution where they will be accessible to specialists, and not hoarded in private collections where they are not always cared for and not always accessible.

(3) I agree as far as the overworked Commission angle is concerned. But I don't believe that the present scheme should continue with an " unofficial " status, which is implied by Dr. Usinger's comment. Much as I dislike some things about the neotype problem, I believe that we shall have it with us more and more, and that we must establish some clear-cut rules to regulate neotypes and to furnish guidance for authors. It is scarcely necessary to point out, however, that not all neotypes have been designated when taxonomists really and truly *needed* them !

(4) Good, though I would go farther in requiring agreement with original information as far as possible.

(5) Impracticable to require that we fully redescribe and figure all 900,000 species (the threat that looms large from your comment 7) already described, in addition to all the new ones coming along ! As for validation under Article 25, could not all neotypes be easily validated merely by a bibliographic reference to the original description ?

(6) I dislike the passive attitude here. I should prefer to require that a specialist consult *before* he designates a neotype (perhaps too idealistic, but lack of sufficient co-operation, and too much of unilateral actions, have been the bane of taxonomy in the past).

(7) Perhaps more emphasis should be placed on careful and full *original* descriptions, with figures where necessary and/or desirable.

Enclosure 5 to Appendix 3 to Dr. Follett's letter

COMMENTS FURNISHED BY PROFESSOR HENRY TOWNES

(For an explanation of the numbers cited in the following document see Editorial Note to Enclosure 4 to the present Appendix.)

Section A

(1) " I am in favour of the recognition in the Rules of the concept of neotypes as a category of type specimen." [Answer given.] Yes.

(2) " I agree with the proposals on neotypes presented by Hemming as summarised in Section A above." [Answer given.] No.

(3) I agree with number 11 and disagree with all others in Section A above.

Section B

(4) " I agree in general with the critique presented in Section B above." [Answer given.] Yes.

(5) I agree with numbers 1-5, 7 and disagree with number 6 (this formality is burdensome) in Section B above.

(6) " I wish to make the following additional comments and I authorise the Committee to incorporate these views in its report and transmit them to the International Commission for consideration at the Copenhagen Congress in 1953."

In regard to museums as correct depositories for *all* type material, the discussion is pointless until a " museum " is defined, and the definition is not intellectually possible without arbitrary restrictions.

Section 11 of Hemming's proposals is opposed to all the other sections. Section 11 alone is sufficient as a philosophy and guide for neotype designation and use. Section 11 recognises priority, first reviser, and any information in the original description and surviving type material. When these principles are recognised, *where* is there any room for other principles or procedures ?

Annexe to Enclosure 5 to Appendix 3 to Dr. Follett's letter

COMMENT BY PROFESSOR HENRY TOWNES IN A LETTER TO DR. FOLLETT

Neotypes, in my opinion, represent only a taxonomist's interpretation of a name and have value only in clarifying his interpretation.

Enclosure 6 to Appendix 3 to Dr. Follett's letter

COMMENT FURNISHED BY PROFESSOR JOHN W. WELLS

The proposals on neotypes by Dos Passos seem very sound and understandable to me. They are clear and simple, and I would recommend them strongly.

Enclosure 7 to Appendix 3 to Dr. Follett's letter

COMMENTS FURNISHED BY DR. JOHN T. ZIMMER

(For an explanation of the numbers cited in the following document see Editorial Note to Enclosure 4 to the present Appendix.)

Section A

(1) " I am in favour of the recognition in the Rules of the concept of neotypes as a category of type specimen." [Answer given.] No.

(2) " I agree with the proposals on neotypes presented by Hemming as summarised in Section A above." [Answer given.] No.

(3) I agree with numbers 1, 2, 4, 10, 11, 15 and disagree with numbers 3, 5, 6, 8, 9, 12 in Section A above. Nos. 7, 13, 14 problematical.

Section B

(5) I agree with numbers 1, 3-7 and disagree with number 2 in Section B above.

(6) " I wish to make the following additional comments and I authorise the Committee to incorporate these views in its report and transmit them to the International Commission for consideration at the Copenhagen Congress in 1953."

I disapprove of the concept of neotypes, but in case there is a successful effort to establish them, I believe they should be under restrictions that will prevent their abuse. Hence the seemingly inconsistent approval and disapproval in my vote.

My disapproval of neotypes is based on belief that they do not truly represent the original concept. If an original name is unidentifiable without the holotype, it should be discarded, not transferred to a new entity that may or may not be identical with the original concept. If it can be identified, no neotype is needed.

Annexe to Enclosure 7 to Appendix 3 to Dr. Follett's letter

COMMENTS FURNISHED BY DR. JOHN T. ZIMMER IN A LETTER TO DR. FOLLETT

I disapprove of neotypes, as indicated on another sheet. If they are adopted, their designation should be made only in a comprehensive review of the group concerned, should be approved by a special committee of experts in the particular field after advance notice through the Commission, and the neotype should be deposited in a public institution of recognised international repute. If a supposedly lost holotype reappears, it should be entitled to its original standing, regardless of dispositions made during its absence. This is one of the points that illustrate the illogical nature of neotypes. In attempting to fix the name on a new concept, some proponents are willing to admit that the rediscovery of the holotype may upset their conclusions, but are not willing to take the consequences. I maintain that if the name is unidentifiable, it belongs in the *nomina dubia* and is not entitled to association with another entity.

APPENDIX 4 TO DR. W. I. FOLLETT'S LETTER OF 21st JULY 1952

EXTRACT FROM THE PROCEEDINGS OF A SYMPOSIUM ON ZOOLOGICAL NOMENCLATURE RELATING TO THE QUESTION OF THE RECOGNITION OF NEOTYPES AS A CATEGORY OF TYPE SPECIMEN HELD BY THE PACIFIC SECTION OF THE SOCIETY OF SYSTEMATIC ZOOLOGY AT THE OREGON STATE COLLEGE, CORNWALLIS, OREGON, U.S.A., ON FRIDAY 20th JUNE 1952

SYMPOSIUM : *Problems of Nomenclatorial Practice Now Under Consideration by the International Commission.* W. I. Follett, presiding

(1) The Question of Whether, and Subject to What Conditions, the Concept of a " Neotype " Should be Officially Recognised in the *Règles.* Robert L. Usinger, University of California at Berkeley.

Dr. Usinger reviewed the historical definitions of the neotype, beginning with that of Kossman (1896) : " a topotype figured or described to replace a specimen which has been lost or destroyed," and later modified by to read : " a specimen, not necessarily a topotype, figured or described to replace a specimen which has been lost or destroyed." With this he contrasted the definition proposed by Hemming (*Bull. zool. Nomencl.*, 7, 1952 : 140) : " a final and irrevocable standard for the identification of a species in cases where there is no surviving type material or where the known surviving type material is insufficient for this purpose."

In the discussion that followed, Dr. G. F. Ferris agreed with Dr. Usinger that the first definition, as later modified, is the one most generally understood by systematists when the term " neotype " is mentioned, and that the definition proposed by Hemming was unwise, in that it opens new views and permits individual interpretations of what constitutes insufficient surviving type material.

Dr. M. W. de Laubenfels raised the question of neotypes of genera, which he believes are needed in his work on sponges.

Dr. J. S. Garth spoke in opposition to any requirement that neotypes be deposited in the institutions which had custody of the original types prior to their destruction. He pointed out that the west coast crustacena types of A. Milne Edwards, Stimpson, Lockington, and Cano were destroyed by a series of catastrophes, and that if Pacific coast workers were to be held to such a provision, they would be working almost exclusively for the benefit of institutions other than their own and in most cases remote from the present centres of activity on Pacific coast Crustacea. Dr. Ferris agreed that such a stipulation was unwise and unnecessary.

With respect to the feared exploitation of the neotype (Hemming, 1952 : 135) and the consequent expressed desire that the sole right to designate neotypes be vested in the International Commission, Dr. Martin R. Brittan inquired whether the Commission has a staff of specialists competent to adjudicate these matters. Dr. Usinger replied that, while the principle of consulting specialists is well established, the number of petitions is already great and the addition of the responsibility of designating neotypes would, in his opinion, seriously overload the Commission and its consultative machinery.

Concerning the proposed stipulations (Hemming, 1952 : 137, 138) that the neotype should conform to the original description and be from the same locality as the type, Dr. Usinger suggested that " the neotype be not inconsistent with the original description with respect to type locality, etc." He also was of the opinion that the law of priority would suffice in cases involving possible duplicate neotypes (Hemming, 1952 : 135).

Regarding the statement (Hemming, 1952 : 140) that the neotype, once established, should take precedence over all surviving type material, Dr. Usinger affirmed that there should be no surviving type material ; otherwise, there would be no necessity for establishing the neotype. The case of loss of the holotype with subsequent rediscovery was raised, Dr. Ferris being firmly of the opinion that a neotype proposed under these circumstances should lose its validity upon such rediscovery of the lost holotype.

Instead of the one year of public notice that would be required by Hemming (1952 : 141) before a neotype could be designated as such, Dr. Usinger proposed that a one-year period of protest be allowed subsequent to publication of a neotype designation. This proposal met with general approval among those present.

The proposal for the restriction of the type locality (Hemming, 1952 : 139) was discussed by Dr. Brittan, who raised the question of extinction of the species in the locality from which the lost type came, and its survival in some other locality. To this Dr. Usinger opined that the present trend was away from restricting type locality in the designation of neotypes.

The distinction made between official and unofficial neotypes and the implication that the latter would have no standing under the proposed new system (Hemming, 1952 : 146) drew forth some of the sharpest comment of the day. It was generally felt that to declare null and void all neotypes proposed before the date of inauguration of the new system would undo much

careful work by many competent specialists who have preceded their colleagues (and the International Commission) in recognising the need for neotypes, and in establishing them.

Although no vote was taken, it appeared to be the consensus of those present (1) that neotypes should be recognised; (2) that if the original type were rediscovered, it should take precedence over the neotype; (3) that the concept of neotypes should not be extended to supplement inadequate types, but only to replace lost types; (4) that neotypes should not be subjected to the exclusive jurisdiction of the International Commission; and (5) that the suggestions embodied in Dr. Usinger's critique of Mr. Hemming's views are generally acceptable.

JOHN S. GARTH,
Provisional Secretary.

DOCUMENT 2/27

By E. RAYMOND HALL
(*University of Kansas, Department of Zoology, Lawrence, Kansas, U.S.A.*)

Enclosure to a letter dated 22nd July 1952

No rules necessary; any author who writes about a given species (or subspecies) in a taxonomic fashion will do well to designate precisely, by Museum Catalogue number or other appropriate means, a particular specimen that he considers representative of the name concerned if there is no holotype.

DOCUMENT 2/28

Statement of the views of the
AMERICAN COMMITTEE ON ENTOMOLOGICAL NOMENCLATURE
Enclosure to a letter, dated 24th July 1952, from
Professor CHARLES D. MICHENER, Chairman

Neotypes (Commission's Reference Z.N.(S.)358)

The neotype concept, originally confined to the replacement of lost types, should not be expanded to cover cases where existing type material is insufficient to provide a standard for the identification of species. Worthy though this might at first appear, it introduces a new subjective element into the picture because there will be differences of opinion as to the adequacy of most type material. One individual may find a type inadequate while another student may discover characters which make it adequate. The action of the " First Reviser " has sufficed in the past to take care of questions of this kind until or unless the type becomes identifiable with further study.

Museums are generally considered to be the rightful depositories for *all* primary types, it being generally agreed that such specimens, upon which scientific work has been based, are the property of science. Nevertheless, this is no more than a recommendation for holotypes, etc., and it is not clear that neotypes are in any way more sacred than holotypes.

Exploitation of neotypes could presumably be prevented just as well in the case of individuals as institutions (just as exploitation of the secondary homonym procedure can be prevented) by action of the International Commission. The difference between the present method of designating neotypes (by individuals) and the proposed method (exclusively by the International Commission) is that at present the individual taxonomist is provided with a procedure that he can use when he needs it, i.e. when he is doing the work. By the other method a formal petition must be submitted to the already overworked Commission. Under the present " unofficial " scheme the Commission would have to act only in the rare instances when actual abuses have been noted and have been submitted to it for adjudication.

Central authority is not needed to avoid competing and duplicate neotypes, since priority of designation is a perfectly satisfactory criterion in such cases ; no central authority should be charged with keeping the museums of the world under surveillance in order to ensure that adequate care is being taken of neotypes. No central authority need decide when to permit departure from original descriptions or original type localities if the Rules were simply to require that neotypes be not inconsistent in characters and type locality with the data provided in the original description.

The requirement that a full description and figures be provided when designating neotypes is fine, but is an " ideal " recommendation, and should not be a rule.

It should be realised that due to the fragile nature of most scientific specimens, nearly all original types will ultimately have to be replaced, in 100 or 1,000 years. Seen in this perspective, the procedure to be set up should be geared to regular *taxonomic practice* rather than to a central authority.

In view of the above comments, the American Committee on Entomological Nomenclature recommends the following rules, modified from those originally drawn up by Mr. C. F. Dos Passos.*

Definition. A neotype is a specimen designated to replace the holotype or the lectotype (both hereinafter referred to as the type) when the type is lost or destroyed.

Rules. After 1st January 195[?], an author may designate a neotype by complying with the following rules :—

A. The type must be lost, or destroyed, and evidence of that fact must be set forth in the publication containing the designation.

B. The designation of the neotype must be published and the neotype must be labelled as such by its designator.

C. Except for inaccuracies in descriptions, the neotype must agree with the original description and any published redescription of the type before its loss or destruction.

D. The neotype must agree also with the first published restriction of the species, even though not accompanied by a lectotype or neotype designation unless this restriction is at variance with the original description or locality.

E. The neotype must be from the original type series (i.e. from among "paratypes") if there is a surviving recognisable specimen in agreement with the first tenable restriction; otherwise it must be (1) from approximately the same locality, if definite, as the type, or in indefinite, from a locality subsequently fixed, (2) taken at approximately the same season, and (3) of the same sex and host as the type, if any or all these facts are known.

F. If the type locality, as originally published, is shown to be false, the neotype must be from approximately the same locality as that subsequently fixed.

G. Neotypes shall not be designated *en masse*, but (except as provided under H below) in a revisionary paper only, to which the designation is relevant and material.

H. Nothing herein contained shall

(1) invalidate any neotype designation published on or before 31st December 195[?], in conformity with these rules and the burden of proving non-conformity shall be upon the author alleging that fact, but such neotype, if published in substantial conformity with these rules, may be republished in conformity therewith in a paper whether revisionary or not, and shall date from the time of the original publication.

(2) validate any neotype designation whenever published, if at any subsequent time the type is found.

*For the original proposals by Mr. Dos Passos see Document 2/13 (pp. 121-127).

I. After 1st January 195[?], a neotype not designated in accordance with these rules shall be null and void, and shall not prevent the subsequent designation of a neotype in accordance with the provisions hereof, but in such event the author shall set forth in detail the reason for claiming that the prior designation is invalid.

J. In the event that two neotypes are designated for the same entity, the first published shall be the valid neotype.

American Committee on Entomological Nomenclature.

CHARLES D. MICHENER, *Secretary.*

DOCUMENT 2/29

By J. CHESTER BRADLEY

(*Cornell University, Ithaca, N.Y., U.S.A.*)

Statement received on 25th July 1952

Neotypes (Reference Z.N.(S.)358)

I express in sequence my views on the three questions asked by the Secretary in paragraph **25** on page 147 of Volume 7 of the *Bulletin of Zoological Nomenclature.*

(**A**) I am strongly in favour of the recognition of neotypes. A contrary course would tend to block progress and precision. In fact while the Commission could withhold official recognition of neotypes, it would be unable to prevent zoologists from establishing them on the same basis that they have been doing.

(**B**) The draft plan is in general good ; but I think it goes too far in trying to establish an ideal.

It must be remembered that prior to 1948 the *Règles* did not recognise any category of type specimens for the fixation of a trivial name. In that respect they were very many decades behind zoological practice. It is only within four years that " neotype " has been any less official than " holotype," " lectotype," or " syntype." Nevertheless there is a distinction in principle between on the one hand " holotype and syntype " and on the other " lectotype and neotype." The former two are factual, being the individual specimen or specimens upon which the original author based his species. The latter involve selection by a subsequent author and therefore the application of judgment to restrict or interpret the intent of the founder of the species. It is appropriate that such selection and restriction or interpretation be restrained, if need be, by regulation.

I shall speak elsewhere of the need for doing this in connection with lectotype.

There is no compulsion (it may not even be desirable) to make the regulations for establishing a neotype more stringent than for establishing the holotype of a new species. Let us not establish a standard so strict that zoologists will not live up to it. I suggest that the provision that a neotype must be newly described (as an individual specimen) and figured be made advisory, not mandatory. As in the case of selection of a lectotype, there are just two things that are *essential* so far as public notice is concerned : (*a*) that the specimen selected as neotype and its labelling be so clearly indicated in a publication that it can be positively recognised by other workers as being the specimen designated, and (*b*) that the selector when doing so make clearly evident the taxonomic species with which he subjectively identifies the neotype. The method of doing this may vary with the circumstances. It should be left to the judgment of the selector. Sometimes it might be best done by publishing an illustration, but again it might be as well done and less expensively by referring to an already published illustration of the species. Certainly to have to publish a figure of *Papilio plexippus* in order to establish a neotype would be wasted expense, since adequate figures exist and can be referred

to, and since the taxonomic sub-species can be clearly identified without reference to any figure. A figure of a sub-species of *Ursus* might be of little use in establishing a neotype for the grizzly bear, but statement of locality and reference to the work of Merriam might be entirely definitive. If the selector cannot perceive the characters upon which authors may eventually rely to separate species or sub-species, the chances are that his illustration will not depict them. The neotype is always the court of last resort, in case questions of discrimination arise. Even if the selector completely fails to convey any idea of the actual taxonomic identity of the neotype, the case is no worse than those of thousands of holotypes of species that have been inadequately described and not restudied.

There exist innumerable ancient trivial names, with no known corresponding type specimens, the descriptions of which apply each to any of several well-known species, but which remain *nomina dubia* because of lack of evidence for applying them to any particular ones. In other cases such names are applied *by tradition* to a particular taxonomic species, but for no other reason. It will be a step toward security if systematists are encouraged to establish neotypes for such nominal species. Almost invariably nothing further would be desirable than to clearly mark and locate the specimen, and then to define the taxonomic species which it typifies in terms of already published work.

In permitting the establishment of such and other neotypes, the Commission will have to meet the problem of how to make sure that the selector has actually exhausted the possibilities of finding existing type material.

In the present world situation a policy will have to be adopted in regard to types that are (*a*) known to exist in or (*b*) may exist in certain or even all countries behind the iron curtain, where they are not available to western scientists. Would the Commission favour requests to establish neotypes in such cases ? Or would it find it a better policy to continue the names involved in them as *nomina dubia*, always with the hope that at some future time the type specimens in these countries may again be available for study ?

I will comment upon the numbered paragraphs in the draft plan: **7.** Agreed and important. **8.** Agreed. It would seem that what is desirable should be set forth in a Recommendation, in general terms, and that to do so would prevent many cases coming to the Commission that would have to be rejected or remanded for modification. **9.** Agreed. **10.** I agree to the necessity, but the proposal to authorise a central body (presumably the I.C.Z.N.) to decide what institutions are acceptable for preservation of neotypes is one that is likely, if adopted, to give rise to bitterness and especially to international ill-will. I once visited the national museum of a certain country. I noted that type specimens of insects had been virtually abandoned, were crowded into drawers of discards, with no recognition or indication of their status. Yet if the Commission were to veto locating neotypes of the fauna of that country in that museum it would be a tremendous blow to the pride of the nationals of the country. Another difficult question that will surely arise: Should it be permissible to locate neotypes in institutions behind the "iron curtain"? The museums of Budapest, Stettin, Konigsberg, Dresden, Berlin, of Russia and of China are not accessible to specialists from the western world. I am far from enthusiastic about seeing the Commission assume such

responsibilities. Is it not enough to lay down in a Recommendation the conditions as to where neotypes ought to be placed and then assume that the author will act responsibly ? If it is deemed necessary for the Commission to decide, then I believe that as far as it could well go would be to submit a series of questions to the institution, asking whether it desired to become a custodian of type specimens in public trust, whether it is prepared to give them adequate permanent care and protection, and to make them available for study to all properly qualified zoologists ? I do not think that it would be wise for the Commission to look behind the face value of the answers. It would probably be desirable, if the plan is put into effect, to establish a list of accepted institutions. **11.** See above. **12.** The necessity is clear. I should like to inquire to what extent this would be carried in certain respects. (*a*) In case the sex of the existing holotype or lectotype is not determinable on the basis of taxonomic knowledge of the time, would it be deemed appropriate to replace it with a neotype of the opposite sex ? Would it permit substituting an adult neotype for the holotype of a species based on larva, in cases where the larva cannot be differentiated specifically, or where it has not been possible to determine correspondence between larva and adult ? What would be the attitude towards establishment of a neotype to replace a holotype on the grounds that the authorities of the institution that possessed the latter would not permit a dissection essential to determination, or would not permit adequate examination (some institutions are known to refuse to allow specimens to be removed from cases for close examination), or, as in the case of some private collections, would not permit any examination to be made ? In case a neotype is established to replace an existing holotype or lectotype, would it not be well to provide that it would have to become the property of the institution in which the replaced type specimen existed ? Otherwise abuse might arise. The establishment of a neotype could be a pretext to get the type away from some institution. The less cases of that sort that have to be met the better, for the Commission cannot be expected to serve as detectives. **14.** The principle could be established that a neotype should come from as near as possible to the locality where holotype or lectotype was collected, if known, or to the locality where some one of the syntypes was collected, and in no case should it come from an area where the species or sub-species that the original holotype or lectotype represents could only doubtfully have come from. **15.** I favour plan (*b*), with the further exception that it should apply primarily to those cases in which the neotype was established to replace a type believed to be lost and not to those where it was at the time known to exist. This for the reason that at times in obscure groups great harm can be and doubtless will be effected by the poor judgment of some systematist in attempting to establish neotypes ; yet we may apparently make out an excellent case for his intent, and if there is no other systematist familiar with the group, his views will not be challenged, but in later years systematists may discover the confusion that has resulted and apply to the Commission to remedy it. **16.** I do not know how to improve on this, and yet if there is only one systematist actively working on a group, it will be impossible to check upon the wisdom of what he proposes. **17.** Twelve months would be satisfactory. **18.** Agreed. It must be born in mind that a neotype is as necessary for a name that is a junior synonym, whether subjective or objective, of another, as for a nomenclatorially valid

name. Furthermore it might be well to recommend that in the case of a name that has never been in wide use, because never fully identified, the best way to dispose of it would be to establish the holotype or lectotype of an older nominal species as its neotype, where circumstances would permit. The effect, of course, would be to sink it as an objective junior synonym. **19.** See my remarks elsewhere concerning Article XXXI. I am not fully in accord with the Secretary's views at this point, unless a holotype or lectotype has been previously established and is now being replaced by a neotype. In such case I fully agree that the latter must be as precisely equivalent in value to the type that it is replacing as humanly possible, and above all things of the same taxonomic species, sub-species and form. But if the syntypes consist of a mixture of species or sub-species and no lectotype has been selected, the species has never been objectively defined. It would seem then to be adequate to recommend to the selector of the neotype that under normal circumstances he avoid choosing a specimen that represents a species or sub-species that someone has removed from the original. Circumstances can arise, however, under which just that would be the better course. For example, assume " a," " b " and " c " are syntypes of one nominal species *E-us y*, but " a " really belongs to one, " b " and " c " to a second taxonomic species; an author (without establishing a lectotype) notes that the description embraces two species, and establishes a new nominal species for specimen " c " (as evidenced let us say by the data on that specimen). Further assume that his action is quite overlooked and that all authors use the name *E-us y* in the sense of specimens " b " and " c," while the species represented by " a " is quite lost track of. In such a case clearly the desirable thing would be to establish specimen " b " or " c " as neotype of *E-us y*, and let the overlooked name *E-us y* fall as a synonym. It would surely be wise, however, to require that any zoologist proposing to select a neotype that represented a form or was the identical specimen that the work of some prior revisee had intended to remove from the species to state his reasons for so doing in asking the Commission to validate the neotype.

The *principle* involved here is like that of selection of a type species of a genus. It may often be wise to select one that has not been made the type of a later genus, but is not required. It is quite the old rejected principle of type by elimination.

The entire argument applies as the equal force to selection of a lectotype, which I discuss elsewhere, and it would seem desirable to deal with most details of selecting lectotype and neotype together and with identical provision.

I quite agree that the no longer necessary powers referred to in Section 19 should be repealed.

I also fully concur that neotypes and not figures are essential for precise definition. The unanswerable argument here lies in the fact that a figure can only depict what the artist chooses to show, while a specimen possesses every characteristic with which Nature has endowed it. Furthermore, figures may be and often are inaccurate.

I should carry this conclusion to the point of revising the regulation concerning holotype, ruling out an illustration altogether; and requiring that

in the case of every species based only on a figure, a neotype must be established before complete objective definition is possible. This does not preclude the fact that species exist which may have no type specimen, but that are so completely known that no type specimen would be of any practical use, domestic animals for example, the human species, etc. Complete objectivity becomes unnecessary.

20. Agreed to, with limitations previously set forth. With the innumerable cases that may arise, I do not think that it will be possible for the Commission to assure itself of these matters from first hand knowledge, or to do more than assure itself that the would-be selector has made an honest effort to solve each problem in the manner most suitable to the individual case, and that no strong dissenting opinion exists. The unfortunate thing is that in many cases no opinion would develop one way or another, perhaps for years, until some other worker began a critical study of the genus concerned.

21, 22 and **23.** Agreed, with some limitation as previously expressed.

24. It does not seem desirable to have two classes of neotypes. It would seem desirable that the official recognition of a neotype should be signalised by some such notation as the Secretary suggests. I believe the proper course is to recommend specialists to recognise and abide by neotypes that have been in the past *published* as established, where there is no serious reason for an opposite course, but at the same time to urgently request them to bring all such selections before the Commission for official confirmation as rapidly as possible.

The Commission should be directed to accept all such previously published neotypes unless there should be strong cause in a given case for not doing so, and not to attempt to apply the several regulations so strictly to them. It must be remembered that these have had the sanction of custom, and prior to 1948 were on the same plane as holotypes, in that neither were recognised by the *Règles*. It would cause quite a furore if the Commission were some day to decide that no holotype had any status until accepted by the Commission, and then try to apply that regulation to the past.

(**C**) Except as noted above, there are no questions not dealt with in the draft plan, that appear to me to require inclusion in the *Règles* in connection with this subject unless perhaps in answering Dr. Frizzell's question about the terms neotype, neoholotype and neoparatype it should become desirable to state that :—

A neotype is a single specimen selected to replace a holotype or a lectotype that has been destroyed or that is believed to have been destroyed, or which is inadequate to permit specific and subspecific determination. The term neotype also applies to a single specimen selected to serve instead of a holotype in case no holotype has ever existed, no lectotype has ever been chosen, and no syntypes exist from which a lectotype can satisfactorily be chosen.

The term neoholotype is rejected as synonymous with neotype.

The term neoparatype if useful to taxonomists is not nomenclatorially objectionable. As such a specimen cannot objectively determine the application of a trivial name, no provision for it is made in these *Règles*.

Annexe 1

SUPPLEMENT TO PRECEDING REMARKS

8. I do not accept the conclusion (last sentence in paragraph 8) that there must be a central authority. I believe that a plain statement of what is desirable and of abuses that should be avoided is adequate. Then the case of any neotype established in flagrant abuse of the proprieties, as thus set forth, could be brought before the Commission for review. Such cases would be few, and could easily be handled. It is not practical for the Commission to handle *all* cases.

20. The preceding paragraph applies more widely here. The longer I consider the matter the less practical does it seem for the Commission to undertake to act on all neotypes. Only the author can know and visualise the taxonomic and anatomical facts—the Commission can only accept or reject the views of others. The *best* results will come from clearly stating everything that is desirable and all that should be avoided, then from letting each author use his own judgment. If he decides to establish a neotype all that he need then do is publish what is required, register the neotype, and receive the assignment of an official number. The Commission will then have only to consider those cases where abuses or faulty judgment raise the question as to whether the neotype concerned should be cancelled.

12. I believe that at the present time it would be unwise to recognise neotypes created to replace an imperfect extant type, or one which is of a sex, stage or form that taxonomists are unable to specifically identify. Discussion on this matter could be invited pending the next Congress after Copenhagen. But I think that a clear demand should arise before provision is included in the *Règles*. As a first step the Commission might be given power in exceptional cases, to deal with names that remain *nomina dubia* because the type, though known, gives no clue to the taxonomic identity of the species. In cases where no systematist who has examined the type claims to be able to positively recognise it, and no one raises taxonomic objection, the Commission might set aside the holotype or lectotype and establish a neotype. In doing this they should be required : (*a*) in case the name is in current use, to establish a neotype in accordance with such usage, or (*b*) in case the name is not in current use, either to sink it as an objective synonym of an older name, by establishing the holotype or lectotype of the older name to be neotype of the name in question, or (under their plenary powers) to suppress the name, if there is no older name with which it could be appropriately synonymised.

Annexe 2

PROPOSED RULES FOR NEOTYPES*

(Reference Z.N.(S.)358)

Definition : A neotype is a specimen identified with a species already described and selected as a standard of reference to replace the lost or destroyed holotype, lectotype or prior neotype of that species. In cases where no holotype was originally designated, no lectotype ever selected, and no syntypes remain from which a lectotype may be selected, the neotype serves in lieu of a lectotype. In any case where the International Commission on Zoological Nomenclature cancels the status of a holotype, lectotype or neotype, that shall be considered the equivalent of its destruction.

Tentative Establishment : A specimen receives tentative status as a neotype only when notice of its selection is published under conditions of publication specified in Article 25 of these *Règles*, and provided further that the following conditions are adhered to :—

(*a*) that the author states his reasons for believing the holotype, lectotype or all syntypes to be destroyed or lost, and in the latter case what measures have been taken to find them.

(*b*) that the neotype is, or at time of selection becomes, the property of a public institution, the name of which is given, or is placed on permanent loan in the custody of such institution.

(*c*) that the specimen be so identified that it can be unmistakably recognised as the neotype. All labels that it bears should be indicated.

(*d*) that the author states his views as to the taxonomic status of the neotype.

(*e*) that if the neotype is to replace a lost lectotype, and syntypes still exist, it shall be chosen from among the syntypes, or the author shall demonstrate either that in his view no existing syntype is of the same taxonomic species as the lost lectotype, or that for some reason it would be futile to establish any one of them as neotype.

Final Establishment : During a period of twelve months following publication of notice of selection of a lectotype the selector may publish a statement withdrawing such selection ; or during the same period any taxonomist finding the selection unsuitable may refer the case to the Commission for decision. If no such action occurs within the prescribed period, the case shall be deemed closed.

If it is impractical to secure actual publication of the notice of withdrawal within the twelve-month period, submission of the manuscript to an editor within that period shall be considered to satisfy the requirement provided

*These are not offered as final or perfected rules, but merely as illustrative of my personal present conclusion as to what is wise at the present time. In reaching them I have been influenced by the views of Messrs. Dos Passos, Usinger and Sabrosky.

notice is published showing the date of submission to have been with the period, a dated copy of the manuscript is submitted to the Secretary of the Commission and actual publication occurs within a further period of twelve months.

Subsequent to the elapse of the above-mentioned first twelve-month period, changes in the status of a specimen selected as a neotype can only be made by the Commission.

Recommendation : Any taxonomist finding a neotype selection unsuitable is urged to correspond within the twelve-month period with the selector, if at all possible, and to endeavour to reach agreement as to the most suitable selection. He should apply to the Commission only in cases where such a course is not practical or agreement cannot be attained.

Priority in Neotype Selection : If two or more specimens are each designated neotype of one nominal species, the ordinary rules of priority shall obtain, each selection dating from the time of its first publication under the preceding rules.

Neotypes Not Objects of Commerce : Neotypes are not subject to sale or barter, except as between public institutions. If it shall be brought to the attention of the Secretary of the International Commission on Zoological Nomenclature that a neotype has been sold or offered for sale or barter by a private individual or a commercial firm, he shall publish in the *Bulletin of Zoological Nomenclature* notice of the automatic cancellation of the status of that specimen as a neotype, without requiring action of the Commission.

Rediscovered Type Material : If type material believed to have been lost or destroyed is rediscovered subsequent to the final establishment of a neotype, it shall automatically replace the neotype. The Commission, upon application, may reinstate the neotype if circumstances render such action desirable, as, for instance, a threat to continuity of established usage. (The wording employed in this section has been suggested to me by Mr. C. F. Dos Passos.)

Recommendations : Individual cases vary in nature to such a degree that the following recommendations cannot be made obligatory. Nevertheless the selector of a lectotype should adhere to each of them unless there is compelling reason for not doing so, in which case he should explain his reasons. Failure to adhere to any one of them may, in appropriate circumstances, be deemed adequate reason for cancellation of a neotype by the Commission.

(*a*) *Locality :* When the precise locality from which the original holotype or lectotype originated is known, subject only to the availability of material, a neotype should be selected from specimens collected from as near to that locality as possible. *In no case may a neotype be selected from a locality which the selector suspects may be outside of the natural range of the species, or nomino-typical sub-species when the species is divided.*

(*b*) *Characters :* The neotype must agree with the original description and any published redescription of the type in all respects that the selector deems of specific or subspecific significance. If the species has as originally proposed been subsequently revised and restricted (even without lectotype designation) the neotype should ordinarily similarly agree with the restricted description.

(*c*) *Sex, Form, Host, etc. :* When the sex, stage, form, etc., of the lost holotype or lectotype are known, and these matters are of significance, the neotype should be equivalent, unless that would destroy or reduce its utility for specific diagnosis. In the case of parasitic species, it is ordinarily desirable that the neotype should have the same host as the original.

(*d*) *Who Should Designate Neotypes :* Neotypes should be designated only by taxonomists engaged in revisionary work on or similar special study of the group concerned ; they should never be designated as part of curatorial routine.

(*e*) *Institution in Which a Neotype Should be Placed :* The selector of a neotype should exercise precaution to make sure that the institution to which the neotype will belong or in which it will be placed on deposit has adequate facilities for the care of type material, and provision for its study by competent taxonomists. If the holotype or lectotype was in the custody of a particular institution the neotype should be placed in the same institution, unless conditions strongly contra-indicate.

(*f*) *Agreement on Neotypes :* A taxonomist intending to designate a neotype should confer with other taxonomists actively working on the group concerned, if any, in order that agreement on the best selection may be attained in advance of publication.

(*g*) *Search for Missing Type Material :* A neotype should be selected only after the fate of all type material has been accounted for, or an exhaustive study convinces the selector that it is hopelessly lost.

(*h*) In indicating his view as to the taxonomic identity of the neotype, the selector should either describe or figure it, or identify it with a description or figure of a species previously published.

Date Rules Become Effective : These rules concerning neotypes become effective upon the date of their adoption by an International Congress of Zoology. Neotypes published prior to their adoption shall be presumed to have been valid as from the date of their publication, but any taxonomist convinced that any such neotype should be cancelled may present his reasons to the International Commission for decision.

DOCUMENT 2/30

Statement of the views of the

ENTOMOLOGISCHE GESELLSCHAFT, BASEL

Extract from a letter, dated 30th July 1952, from M. HENRY BEURET

(Note :—The numbers cited in the following statement are the numbers allotted to the paragraphs in the paper on the subject of neotypes by the Secretary to the International Commission on Zoological Nomenclature published in March 1952 in Double-Part 5/6 of Volume 7 of the *Bulletin of Zoological Nomenclature.*)

Vol. 7, Parts 5/6

No. **1-6.** Rien à remarquer.

No. **7-14.** Nouse sommes d'accord avec la création de " néotypes," aux conditions suivantes :—

1. qu'ils correspondent autant que possible à la description originale, éventuellement à la figure origininale ;
2. qu'ils proviennent si possible de la " type locality " (qu'ils soient donc des topotypes) ou d'un biotope correspondant a la " type locality " et pas trop éloigné de cette dernière ;
3. qu'il soit exclu que l'on puisse établir des neotypes pour des raisons lucratives ;
4. que l'établissement de néotypes soit subordonné à l'autorisation d'un office central qui devra établir une liste officielle des néotypes et la compléter au fûr et à mesure ;
5. que les néotypes soient déposés dans un muséum et accessibles aux spécialistes, comme vous le proposez ;
6. *qu'aucun néotype ne puisse être établi avant qu'un Congrès international ait fixé éxactement les conditions sous lesquelles un néotype pourra être établi.*

No. **15.** Acceptons l'alternative *b*.

No. **16-23.** Rien à objecter.

No. **24.** Les *néotypes non officials* créés jusqu'au moment où des néotypes officiels seront admis devraient, pour autant qu'ils aient une raison d'être, être *transformé en néotypes officiels.*

No. **25.** Question 1 = oui
2 = voir remarque : ci-dessus
3 = non.

DOCUMENT 2/31

By W. E. CHINA, M.A., D.Sc.
(*British Museum* (*Natural History*) *London*)

Ref. Z.N.(S.)358

(1) Extract from a letter dated 20th August 1952

I am not in favour of the recognition in the *Règles* of the concept of neotypes as a category of type specimen.

(2) Extract from a letter dated 19th September 1952

If neotypes are recognised officially I should certainly be interested in the discussion to ensure the maximum of safeguards against abuse of the regulation. I particularly agree with your suggestion that when types are destroyed the museum suffering the loss should have the first right to fix neotypes. We must ensure that the commercially minded individual is not allowed to establish neotypes wholesale with a view to their sale at some later date or that some very active museum curator should not be allowed to build up his collection by establishing hundreds of neotypes of species of which he knows the types to have been lost or destroyed.

DOCUMENT 2/32

By H. B. WHITTINGTON

(*Department of Invertebrate Palaeontology, Museum of Comparative Zoology at Harvard College, Cambridge, Massachusetts, U.S.A.*)

Enclosure to a letter dated 26th August 1952

RECOGNITION OF NEOTYPES

While I have in my published systematic work proposed two neotypes, I consider now that this step was probably ill-advised. As you point out, in fossil species the need for a neotype arises when either the type material is lost or when it is too incomplete for a full description. The need seems great when the species has been designated as type of a genus. However, there is so much uncertainty about the selection of a neotype of a fossil species—whether it is from the original locality and horizon—that I consider it best no longer to use the name of a species of which the type material is either lost or inadequate. I consider this especially advisable if the species is a type. I, therefore, join my entomological colleagues in this museum* in urging that neotypes should not be recognised and that no provision be made in the *Règles* for such recognition.

DOCUMENT 2/33

By K. H. L. KEY

(*Commonwealth Scientific and Industrial Research Organisation, Division of Entomology, Canberra, Australia*)

Extract from a letter dated 4th September 1952

(Note :—The main portion of the above letter was concerned with the problem of the emendation of names, on which a separate note has been submitted by Dr. Key and which is one of the documents included under Case 5 in the Copenhagen Series.

I have studied your proposals in connection with the other six questions referred to you by the Thirteenth Congress, and in general I find myself in hearty agreement with them.

*See Document 2/10 (pp. 116-117).

DOCUMENT 2/34

By J. BALFOUR-BROWNE, M.A.
(*British Museum* (*Natural History*) *London*)

Enclosure to a letter dated 25th September 1952

On the Question Whether " Neotypes " Should be Recognised in the " Règles " as a Category of Type Specimen

Vol. 7, pp. 131-147

7. Agree that neotype should be deposited in a museum or other public institution.

8. Agree.

9. The duplication of neotypes could be prevented by the definition of what constitutes a neotype—which should be a single specimen which is to be accepted as the type specimen of a species of which the original type (holotype) specimen has been lost or destroyed.

10. Agreed.

11. If an adequate description of the lost or destroyed holotype specimen already exists and a specimen is designated as a neotype it must accord with the original description as nearly as possible as is envisaged in *13 of your paper. A further full description would seem to be supererogatory.

12. I would only agree to the designation of a neotype if the holotype or lectotype was completely lost or destroyed. Any other ground would be dangerous as, for example, due to an erroneous determination of sex of a partly defective specimen. A faulty examination might lead to the conclusion that a holotype was a defective female whereas it was actually as more careful study showed a partly defective male which still possessed the truly definite characters of the species. This could lead to a totally wrong conception of species, and if a neotype is created under these circumstances the situation is insupportable. There is also the case of a holotype being entire but in such fragile condition that any attempt to remount it might be disastrous, but it might be possible to obtain the essential characters by means which broke up the specimen—this would *not* justify creation of a neotype.

If a figure of the essential specific characters (e.g. aedeagus) of a destroyed holotype is extant that figure should be accepted as the neotype even if it is only a portion of a specimen.

13. Agreed, see 11 above.

14. This is not always possible. A Fabrician species might be from the West Indies without specifying which. Agreed that it should, *if possible*, be from the same locality or horizon.

15. As I disagree that a neotype and holotype or lectotype can be in existence at the same time I can only agree in part—that a neotype will take the same precedence as a lost or destroyed holotype or lectotype possessed and will immediately lose that precedence if a " lost " holotype or lectotype is re-discovered even if this should necessitate a change in conception of a species.

16. I agree with the need for the publicity suggested, but would regard it as essential if a full description of a neotype is required to replace any inadequately described lost or destroyed holotype that the description must be published with the application or it would be impossible for interested specialists to express an opinion on the merits of the application.

17. Agreed.

18. This would be covered by 16 above.

19. Clearly essential, but I would not at any time agree to the setting aside of any decision of the Commission under the circumstances quoted for *Papilio plexippus* Linnaeus, 1758.

The power of decision should clearly be irrevocable.

20. If an application for the designation of a neotype *must* be made to the Commission it is clear that the Commission alone is competent to give a decision and is therefore in possession of the exclusive right desired.

21. No comment.

22. Agreed.

23. Agreed.

24. No. All neotypes " unofficially established " at the time of the introduction of the revised *Règles* should be submitted for verification under the agreed procedure. A rejected " unofficially established " neotype would automatically lose all type status just as much as a specimen proposed under the *Règles* as suggested but which is rejected by the Commission. If any application to the Commission for designation of a neotype ignored an " unofficially established " neotype it is extremely likely that the publication of the application would expose the oversight. The suggestion that there can be an officially established and an unofficially established neotype coexistent is insupportable.

DOCUMENT 2/35

By THERESA CLAY, B.Sc.
(*British Museum (Natural History), London*)

Neotypes (Ref. Z.N.(S.)358)

See *Bull. zool. Nomencl.* **7** (5/6) : 133-146. 1952.

1. I am in favour of the recognition in the *Règles* of the neotype as a category of type specimen.

2. I am in general agreement with the draft plan submitted in pages 133-145 with the following comments on alternative suggestions.

Paragraph **8,** p. 135.

I agree that it is quite impracticable to make it essential to select neotypes from material possessed by the institution in which the original type material had formerly been deposited, nor do I think that it should be a proviso that

the neotype must be deposited in the above-mentioned institution, but it might be recommended that a neoparatype (if there is such a category) should be, where possible, presented to the original institution.

Paragraph **12,** p. 137.

I agree that a neotype should be erected even where part of the original type material is in existence, if this is unrecognisable. There is also the case to be considered where the holotype belongs to a sex, say female, of a species which is at the present time only recognisable in the male.

Paragraph **14,** p. 138.

In the case of parasites it may be essential to ensure that the neotype is from the same subspecies of host. Now that the trend in bird systematics is to widen the concept of subspecies so that many forms formerly recognised as species and with distinct species of parasite, are now considered as subspecies. It is therefore important that the same subspecies is selected as host of the neotype. On the other hand there are cases where the host form is only of doubtful subspecific value and in this case the neotype could be taken from a closely related subspecies. This is a point which could presumably be settled by the specialists in the subject when any given neotype was put up for consideration.

Paragraph **15,** p. 137.

I think it is most important that (*a*) should be put into practice, that is that once a neotype has been established, all surviving type material known or unknown should be deprived of its status as such.

Paragraph **19,** p. 142.

This I think is most important, namely that the selection of a specimen for neotype does not run counter to any previous restrictions or restricted identifications.

Paragraph **24,** p. 145.

I consider that the present unofficial neotypes, if they fulfilled the requirements for the establishment of neotypes, should take precedence over an application by another author for the establishment of a neotype for the same species, even if the unofficial neotypes had not yet been put before the Commission. It would have to be declared to the Commission during the proposed 12-month period, in the same way as type material believed to be lost must be declared.

DOCUMENT 2/36

Statement furnished by the AMERICAN SOCIETY OF PARASITOLOGISTS

Extract from a letter dated 8th September 1952

The American Society of Parasitologists at its meeting in November of 1951 appointed a committee to investigate the controversy that arose following the announcement that " far-reaching decisions in regard to zoological nomenclature (had been) taken by the Thirteenth International Congress."

In undertaking its assignment the *Bulletin of Zoological Nomenclature* was studied and the requests for advice in Volume 7 were noticed. The committee reached a unanimous decision on only three points.*

(2) The Committee does not feel that the recognition of lectotypes and neotypes should be designated to the International Commission because such a function involves more than a decision on purely nomenclatorial issues.

In submitting these comments to you the Society wishes to express its gratitude to you and the entire International Commission for the considerable effort that has been expended in behalf of zoological nomenclature.

Signed for the Society by its Committee.

ALLEN McINTOSH.
D. H. WENRICH.
G. W. WHARTON, *Chairman.*

*The other Resolutions referred to here are reproduced at the appropriate points in the Copenhagen Series.

DOCUMENT 2/37

By A. E. PARR
(*The American Museum of Natural History, New York*)

Extract from a letter dated 27th March 1953

You are probably also aware that a committee of the International Council of Museums, under the chairmanship of Dr. Swinton of the British Museum, had a meeting at Oxford last summer considering various problems related to type specimens, which are of concern in relation to nomenclature. One of the proposals that interest me very much is the proposal to establish some sort of machinery for having a type specimen that has been destroyed beyond practical usefulness, or for which there is adequate evidence that it has in all probability been lost, declared null and void so that a new type can be established in order to create stability of nomenclature. I believe it is the intention that this proposal should be presented at Copenhagen.

PURCHASED
11 JUL 1953

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21 JUL 1953

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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BULLETIN OF ZOOLOGICAL NOMENCLATURE

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CASE No. 3

DOCUMENTS RELATING TO THE QUESTION OF THE REFORM OF THE PROVISIONS IN THE "RÈGLES" RELATING TO THE NAMING OF FAMILIES AND LOWER SUPRAGENERIC GROUPS

DOCUMENT 3/1

Origin of the Present Investigation

Editorial Note.—The present investigation into the question of the reform of the provisions in the *Règles* relating to the naming of families and lower categories of suprageneric rank was undertaken in response to an invitation given in July 1948 when the International Commission on Zoological Nomenclature at its Paris Session had under consideration the question of consolidating into the *Règles* the interpretations of the existing provisions relating to the foregoing subject rendered by it in various of its *Opinions*. The Commission then agreed to recommend (1950, *Bull. zool. Nomencl.* **4** : 138) :—

> " (1) that the Secretary to the Commission should be invited to make a thorough study, in consultation with interested specialists, of the problem of the nomenclature of Super-Families, Families, Sub-Families and Tribes and to submit a Report thereon, with recommendations, for consideration by the Commission at their meeting to be held during the next (XIVth) meeting of the Congress, with a view to the submission by the Commission of proposals for the insertion in the *Règles* of comprehensive provisions dealing with this subject."

2. The foregoing recommendation, with other recommendations, was submitted to, and approved by, the Section on Nomenclature of the Thirteenth International Congress of Zoology at its Second Meeting held on 24th July 1948 (1950, *Bull. zool. Nomencl.* **5** : 67, 76).

DOCUMENT 3/2

General Consultation With Specialists and Issue of an Appeal for Advice in 1952

Editorial Note.—Attention is drawn to the review prepared by the Secretary to the International Commission on Zoological Nomenclature of the problems involved in the naming of Families and lower categories of suprageneric rank which was published in March 1952 (*Bull. zool. Nomencl.* **7** : 61-94). This review contained an appeal to specialists to assist in the present investigation by furnishing statements of their views on the action which it was desirable should be taken. It was in response to this appeal that the majority of the documents now submitted was furnished.

DOCUMENT 3/3

The Question Raised by Dr. Harold W. Manter Regarding the Name Properly Applicable to the Family Containing the Genus " Dissotrema " Goto & Matsudaira, 1918 (Trematoda)

Editorial Note.—Attention is drawn to the application submitted to the International Commission by Dr. Harold W. Manter in 1935 in regard to the name properly applicable to the Family containing the genus *Dissotrema* Goto & Matsudaira, 1918 (Trematoda). The point here was (1) that, after the Family Name DISSOTREMATIDAE had been established for the above genus, it was found that the name *Dissotrema* was an objective junior synonym of the name *Gyliauchen* Nicoll, 1915, but that the author who recognised this synonymy (Goto, 1919), while taking the view that this genus required to be placed in a separate Family, did not rename the Family GYLIAUCHENIDAE, (2) that later Fukui (1928) placed the above genera in the same Family as *Opistholebes* Nicoll, 1915, and gave the name OPISTHOLOBETIDAE to the Family which he so recognised.

Dr. Manter's application in regard to the foregoing case was published in 1947 (*Bull. zool. Nomencl.* **1** : 197-198). When in Paris in 1948 preliminary consideration was given to this case by the International Commission, it was decided that a decision on it should be deferred until after the completion of the general review of the problems involved in the naming of Families, the institution of which had already been decided upon, it being agreed at the same time that the papers relating to this case should form part of the documentation for that review (1950, *Bull. zool. Nomencl.* **4** : 401-402).

DOCUMENT 3/4

By H. BOSCHMA

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Examples of Homonymous Family Names

(1) Enclosure to a letter dated 30th January 1946

The names CYPRINIDAE, type genus *Cyprinus* Linnaeus, 1758, and CYPRINIDAE, type genus *Cyprina* Lamarck, 1818, both seem to have been in constant use for about a century. It is rather difficult to trace the authors of family names, as these are rarely mentioned in literature.

Günther (1868, *Cat. Fishes Brit. Mus.* **7** : 3) mentions Agassiz (1843, *Rech. Poiss. foss.* **5**) as author of the family name CYPRINIDAE (fishes).

Jeffreys (1863, *Brit. Conch.* **2** : 297) mentions Geinitz as author of the family name CYPRINIDAE (molluscs). In Agassiz & Strickland (1852-1853, *Bibl. Zool. Geol.* **3**) and in Carus & Engelmann (1861, *Bibl. Zool.* **2**) papers by Geinitz are cited dating from 1837 to 1861.

Bucquoy, Dautzenberg & Dollfus (1892, *Moll. mar. Roussillon* **2** : 313) state that Pictet (1835, *Traité Paléont.* **3** : 463) established the family CYPRINIDAE (molluscs). They add (*l.c.*) : " Le nom de *Cyprinidae* ne peut être conservé pour cette famille, car il a été employé plus anciennement pour désigner une famille de poissons." As a substitute for CYPRINIDAE (molluscs), they use the family name ISOCARDIIDAE, a name which they prefer to GLOSSIDAE, which appears to have been used for the same family by Stoliczka & Cossmann (the name *Glossus* had been proposed to replace *Isocardia*).

It is not advisable to use the name ISOCARDIIDAE instead of CYPRINIDAE. The genera *Cyprina* and *Isocardia* form rather different groups, so that some later author may be inclined to place these genera in separate families.

However this may be, to avoid confusion it is necessary to restrict the family name CYPRINIDAE to one of the two groups of animals, fishes or molluscs.

The CYPRINIDAE (fishes) form a group of animals of economic importance, so that the literature on this subject is much more extensive than that dealing with the CYPRINIDAE (molluscs). It is, therefore, advisable to restrict the family name CYPRINIDAE to the group of fishes.

In *Opinion* 140 (MEROPIDAE and MEROPEIDAE) an elegant solution of the difficulty could be found ; here the case is less simple. Perhaps the name CYPRINAIDAE might be proposed, although this name is not rigidly derived from the stem of the name of the type genus *Cyprina*.

(2) Letter dated 9th January 1950

Besides the case of MEROPIDAE, now settled by the Commission, and that of the name CYPRINIDAE, now brought before the Commission, there are two more interesting cases of identical Family Names :—

(1) The genus *Metridium* (Anthozoa, Actiniaria), Family Name, METRIDIIDAE ; the genus *Metridia* (Crustacea, Copepoda), Family Name, METRIDIIDAE.

(2) The genus *Gerris* (Insecta, Hemiptera), Family Name, GERRIDAE ; the genus *Gerres* (Pisces, Acanthopterygii), Family Name, GERRIDAE.

DOCUMENT 3/5

By CURTIS W. SABROSKY

(*of (at the time when the following paper was written) the U.S. Public Health Service, Manning, South Carolina, U.S.A.*)

Extract from an enclosure to a letter dated 14th March 1946

The Correct Names for Families in Zoology*

Family names are among the most widely used of all the technical names for animals or groups of animals. Introductory textbooks are commonly organised on that basis, beginning students learn to classify the subjects of their study by them, and specialists in non-taxonomic fields may recognise few names beyond those of families.

*The principles discussed in the paper apply equally to subfamily names, but the discussion has been centered on families to avoid the cumbersome phrase " family and subfamily names." Further, because of the large number of names referred to, almost all references have been placed at the end of the paper in order not to interrupt the continuity of the discussion. Aside from the citation of their dates, references for generic names have not been given because in the light of *Opinions* 133 and 141 they are not essential to the arguments in this paper. [*Ed.*—The bibliographical references referred to by Dr. Sabrosky have not been reproduced here.]

Nowhere is the above more true than in entomology, because of the vast numbers of genera and species involved. Whereas the ornithologist, for example, can recognise and give both generic and specific, and perhaps even subspecific, names for the birds in his region, the taxonomic entomologist often does well to know a reasonable proportion of the family names of the insects of his area, outside of the common genera and species and those of his own particular speciality. The economic entomologist will recognise species of economic importance, but beyond that will rely mainly on group names for the various forms that he meets.

Many family names, particularly in entomology, have become quite generally used and understood as common names, not only by taxonomists, but also by biologists, general zoologists, science teachers, etc. Such names as tabanids, tipulids, and sarcophagids have become nearly as well known as their counterparts of horseflies, crane flies, and flesh flies. In some cases, such as the parasitic flies of the family TACHINIDAE, there exists no common name for the group other than that founded upon the family name itself. In other cases, it is convenient to use a common name based on the family name, such as the noctuid moths or NOCTUIDAE (cutworms, stalk borers, earworms, etc., now known as the PHALAENIDAE), either because of the diversity of common names for groups of species within the family, or because the common names apply to larval forms and not to adults.

Obviously, therefore, the changing of family names may have more extended repercussions than the changing of generic or specific names. No matter how vehemently specialists may argue for or against changes in names, they will have to admit that, because of their specialised knowledge and training, no change will be likely to cause them more than temporary annoyance or occasional slips in their book-keeping. However, to specialists in other fields of biology, to students of other sciences, and to non-technical workers, the changing of familiar and long-recognised names, and the continued use of conflicting names by different specialists, contribute not only to confusion, but to a low regard in some quarters for both taxonomy and nomenclature. For example, the pictured-winged flies of the family long known as the TRYPETIDAE are commonly called the trypetids, and they are widely known and recognised because of such common species as the cherry fruit flies, the apple maggot or "railroad worm," the Mediterranean fruit fly, the round golden rod fall maker, and many others. Yet the appearance of such names for the family as TRUPANEIDAE, TRYPANEIDAE, EURIBIIDAE, and TEPHRITIDAE, leaves the average reader only bewildered.

Some changes are of course inevitable, such as the necessity of avoiding homonyms in order to prevent the confusion of having identical names in use contemporaneously for two different groups. In many cases, however, we need prompt action by the International Commission on Zoological Nomenclature in order to clarify certain general principles and to bring system into the often confusing terminology of family names.

Articles 4 and 5 of the International Rules of Zoological Nomenclature, simple and direct as they seem, have been the subject of considerable dispute and conflicting interpretations. Some of the doubts about family names have been resolved by *Opinions* 133 and 141, published in 1936 and 1943 by

the International Commission. However, in spite of the clear-cut statement of principles contained in *Opinion* 141, there still remain several points of contention. Even though in some cases the answer may seem obvious, or the probable answer is indicated by the discussion in the above or other *Opinions*, nevertheless a more comprehensive statement of principles by the International Commission would clear up certain difficulties that now exist in the nomenclature of family names, as indicated in the following discussions.

1. Shall a family name, based on an included generic name, have priority and authorship from the date of its first proposal for a super-generic group, whether originally called a family, or stirps, or etc., and regardless of the patronymic ending employed?

The use of " -idae " as a uniform patronymic ending for family names was first advocated by William Kirby in 1813. Although the idea soon found favour in many quarters, it was naturally many years before it was consistently and universally applied. Meanwhile, authors used such terminations as "-ites," "-ides," and "-ina." In many cases, these were called " family " or " subfamily " ; in others, " stirps," " section," " natural order," etc.

Many of our best-known family names date from this period when there was no uniform family patronymic. The critical study of the origins of these names will sometimes reveal that insistence upon the " -idae " ending for determining priority would overthrow long-established names in favour of unknown or obscure ones. In the two-winged flies (Class Insecta, Order Diptera), for example, a number of our familiar family names were apparently first used by Edward Newman (1834), who consistently used the suffix " -ites " to designate his " natural orders " (=modern families) in such combinations as Bibionites, Psychodites, and Chironomites.

In fairness to early taxonomists, who based their super-generic names on included genera, but merely with different endings to designate the groups, it is believed that the question should be answered in the affirmative.

A suggestion to this effect, in the nature of a proposed amendment to the International Rules, was also submitted to the Commission some years ago (Cf. *Science* **68** : 102-104, 1928 ; *Ent. News* **40** : 332, 1929), as was the principle discussed in the third section.

2. When a family name is changed because the name of the type genus is found to be a homonym, must the new family name be founded upon a new generic name replacing the preoccupied one, or upon the next available generic name?

Although it is believed that the answer will favour the first alternative, it would be well to have a clear and unequivocal statement. The continuity of the type concept is important, when a certain generic group is used as the standard of reference for a family. If a preoccupied generic name which is a familiotype (type genus of a family) should be replaced by a name which is not isogenotypic with it, then the new family name might conceivably be based on a group of species which might sometime be recognised as quite

different from the original type genus. We would then find that the family name would pass to the new segregate, and we would be forced again to rename the original and long established genus and the family name founded upon it.

To cite a definite example, a large and common family of picture-winged flies, similar to the trypetid fruit flies in appearance and size, has been well known for years under the name ORTALIDAE. Not until 1932 was it pointed out that the generic name *Ortalis* Fallén (1810) is preoccupied by *Ortalis* Merren (1786) in ornithology. Two authors almost simultaneously proposed new names for the family, OTITIDAE and PLATYSTOMIDAE.* In each case, the name is based on an entirely *different* genus than the original ORTALIDAE, thus changing the family's type standard. The choice of PLATYSTOMIDAE was particularly unfortunate, since it is based on a genus in another subfamily from the original *Ortalis*, and one which is recognised by some authors as a family in its own right.

3. Must a family name be changed when its type genus is found to be a synonym?

There is no question that, in order to avoid possible confusion, a family name must be changed when the name of the type genus upon which it is based is found to be a homonym.

Changing with synonymy is not so convincing. A homonym is forever dead: it was stillborn and cannot be revived; but it is always possible to resurrect and reinstate a synonym. A homonym engenders confusion because it has the same name as some other animal group; a synonym is a distinctive name in itself, but merely superseded by an older name, sometimes only as a matter of opinion. A synonym is always present in the literature on the family, and a family name based on it would always represent the same type concept, regardless of any changes in the prevailing opinion of its status and synonymy. It is not possible to foresee the trends of future revisionary work and the results of the discovery of thousands of new species. Genera may be combined, later separated, perhaps recombined, with refinements in classification, discovery of annectant species, etc. If a family had to be renamed because of synonymy, there might be frequent changes or vacillation in the family name.

Two examples may be cited by way of illustration:—

(*a*) In the order Diptera (Insecta), the Meigen (1800) generic names (which were first brought to the attention of dipterists by Hendel in 1908) under the Rules supplant the well-known and century-old names of Meigen (1803) for many common genera that gave their names to large and cosmopolitan families. Assuming for present discussion that the 1803 generic names must be changed (which is opposed by some workers), why should we necessarily have to change the familiar CHIRONOMIDAE to TENDIPEDIDAE, or TACHINIDAE to LARVAEVORIDAE, for example?

(*b*) The so-called false crane flies were known for years under the family name RHYPHIDAE, based on *Rhyphus* Latreille (1804). Not until the early 20th century did authors change to ANISOPIDAE (or ANISOPODIDAE) based on

*Based on *Otites* Latreille, 1804, and *Platystoma* Meigen, 1803.

Anisopus Meigen (1803) with *Rhyphus* as synonym. Following Hendel (1908), however, some workers changed to PHRYNEIDAE, based on *Phryne* Meigen (1800) (=*Anisopus* Meigen, 1803). For the next few decades, dipterists were divided on the use of the 1800 names, and both PHRYNEIDAE and ANISOPODIDAE existed in the literature. Finally, as a crowning touch, careful research into the place of the 1800 names in the taxonomic structure (Stone, 1941) revealed that both *Phryne* and *Anisopus* were synonyms of the still older but hitherto overlooked *Sylvicola* Harris (1776). Thus the family name can once more be changed, to SYLVICOLIDAE, which incidentally is a name once used in ornithology for the American wood warblers. How much senseless change would have been avoided, through the years of changing synonymy, by the continued use of the original name, RHYPHIDAE !

4. Is a family name to be rejected as a homonym of another family name?

It is possible that each case might have to be considered on its own merits, but the statement of a general principle should help in many situations. Two problems will illustrate different ways in which family homonyms may arise.

(*a*) In the preceding section, the family of the false crane flies was found to have had four different family names as a result of successive changes in the recognised synonymy of the type genus. Although the name SYLVICOLIDAE has not yet been noticed in the literature, it is the logical step if the nomenclature of family names continues as it has in the past. However, the name SYLVICOLIDAE has already been used in ornithology for the American wood warblers, although later abandoned when its type genus (*Sylvicola* Swainson, 1827) was found to be a homonym. Does this existence of a previous though now unused SYLVICOLIDAE preoccupy the use of the name ?

(*b*) Are identical family names, which are based on similar but not identical (and therefore not strictly homonymous) generic names, to be considered homonyms ?

For example, in the " Statement of the Case " in *Opinion* 153 (p. 199) it was pointed out that a situation appeared to require the use of the name PSILIDAE for a family of bethylid wasps, based on *Psilus* Jurine (1801). Fortunately, with the suppression of the " Erlangen List," the name *Psilus* ceased to be available, for otherwise we might have had a family PSILIDAE in the Hymenoptera, based on *Psilus* Jurine (1801), and the identical name PSILIDAE in the Diptera, based on the valid and distinct name *Psila* Meigen (1803). As a similar situation, there is a family name LARIDAE both in Aves and in the Insecta (Coleoptera), based respectively on *Larus* Linnaeus and *Lara* Leconte.[3] What shall be done with family names based on valid generic names which differ only by ending in -us and -a, where the family names derived from the stem of the name of the type genus are inevitably identical ?

[3]I am indebted to Dr. E. Gorton Linsley for calling my attention to the case of LARIDAE, and also to the fact that family names with a similar sound but different forms are LARRIDAE in the Hymenoptera (from *Larra* Fabricius), and LARIIDAE in Coleoptera (from *Laria* Scopoli). The last two names undoubtedly are sufficiently distinct, for we have the example set by the International Commission in *Opinion* 140, in which official sanction was given to the names MEROPIDAE and MEROPEIDAE, respectively based on *Merops* Linnaeus and *Merope* Newman.

5. What spelling is to be considered proper and official in the case of certain kinds of family names that have appeared in both short and long forms?

A few examples from the Diptera will suffice :—

ACROCERIDAE or ACROCERATIDAE EMPIDAE or EMPIDIDAE
TRICHOCERIDAE or TRICHOCERATIDAE DOLICHOPIDAE or DOLICHOPODIDAE

Inasmuch as *Opinion* 36 (p. 85) called attention to a change from DIOXOCERIDAE to DIOZOCERATIDAE, it is to be presumed that the longer form, based on the full stem of the name of the type genus, is the proper and correct spelling under Article 4 of the Code. It would promote uniformity, however, if the Commission would review the interpretation of the stem of the names of type genera where the proper combining form is not apparent in the generic name itself (as in *Acrocera*, *Empiz*, *Dolichopus*, *Termes*, *Leptocerus*, etc.).

An " Official List of Family Names in Zoology "

A statement of general principles from the International Commission would aid in solving many problems in the nomenclature of family names. Even beyond this, however, it is believed that the preparation of an Official List of such names would do much to promote stability and uniformity in the terminology of these widely used names.

In addition to a request for the statement on general principles, therefore, it is proposed that the International Commission on Zoological Nomenclature, with the aid of special committees of specialists on the various groups of animals, undertake the preparation of an " Official List of Family Names in Zoology." When cases are discovered that appear to warrant Suspension of the Rules in order to prevent greater confusion than uniformity, these can then be promptly submitted to the Commission for action.

The idea is by no means new, though not previously proposed as an " Official List." At the Fourth International Congress of Entomology (Ithaca, N.Y.) in 1929, in the discussion of a paper by Dr. A. L. Melander, Dr. Karl Jordan expressed the view that family names should be stabilised by means of a list of " nomina familiarum conservanda " prepared by a committee for each large order.

Undoubtedly, the difficult cases in family names would eventually be discovered and settled in due course. Rather than wait for the haphazard discovery of problems and the slow accumulation of approved names, however, it is believed that more good would result from the active research and preparation of a comprehensive list that would be a contribution to stability and an orderly nomenclature.

DOCUMENT 3/6

By L. W. GRENSTED, D.D.
(*Oriel College, Oxford University*)

Enclosure to a letter dated 5th October 1947

EDITORIAL NOTE.—The following paper was published in June 1947 in *Ent. mon. Mag.* **83** : 137-141.

On the formation of Family Names

If we are ever to reach security and uniformity in nomenclature it is absolutely necessary, first, that the *Opinions* of the International Commission,

when issued, should be accepted and followed without question, second, that where the Rules upon which those *Opinions* are based are inadequate it should be open to the Commission to re-interpret or to revise them, and, third, that the procedure of the Commission should be such as to inspire confidence in its learning and scientific integrity, as well as in its administrative efficiency and its ability to settle disputed problems with reasonable speed. In what follows I am going to argue that the decision given in *Opinion* 143, establishing the family name TINGIDAE as correct, has implications which are far-reaching and which, if accepted, will greatly simplify one of the more awkward problems as to the formation of such names. I propose, in fact, to treat the *Opinion* in legal fashion, as providing us with case law, fixing the interpretation of Article 3 of the International Rules, and thereby enabling us to determine the meaning and application of Article 4. The argument applies more especially to generic names with alleged i-stems, but it may appear in the end that it has a wider application. If in what follows I seem to suggest some criticism of the work of the Commission, it is not with any desire to undermine confidence in its findings, and certainly not without recognition of the great practical difficulties under which its work is carried on. Entomologists everywhere are under a very great debt to the Commission, and to its Secretary, Mr. Hemming, in particular. It is in the hope that discussion of its work may help to speed the plough that I have written what follows.

It will be convenient to have before us the two relevant Articles of the Rules :—

Article 3.—The scientific names of animals must be words which are either Latin or Latinized, or considered and treated as such in case they are not of classic origin.

Article 4.—The name of a family is formed by adding the ending *idae*, the name of a subfamily by adding *inae*, to the stem of the name of its type genus.

It will be noticed that Article 3 does not determine what is meant by Latin, which, as a living and changing language, has a history of more than two thousand years, though it certainly suggests that what is commonly called " classical " Latin is the norm. But it does lay down quite clearly that such names as *Empis* or *Anthocoris*, however Greek their origin, are to be " considered and treated " as Latin. And this certainly applies when they are being used as the basis for the formation of family names.

So much by way of Preface.

Opinion 143 of the Commission on Zoological Nomenclature reads as follows :—

On the Method of forming the Family Name for *Tingis* Fabricius, 1803 (Insecta, Hemiptera)

SUMMARY.—The family name for *Tingis* Fabricius, 1803 (*Syst. Rhyng.* : 124) in the Hemiptera is TINGIDAE.

The case submitted by Dr. A. C. Baker in January 1923 is set out in these terms :

TINGITIDAE, TINGIDAE or TINGIIDAE

Dr. W. J. Holland (1922, *Science* **56** : 334-5) contends that Fabricius intended to use the word *Tinge*, the Latin equivalent of the Greek word *Τίγγις*, of which the stem is Tingit. He therefore makes the family name TINGITIDAEl

Dr. H. M. Parshley (1922, *Science* **56** : 449) contends that Fabricius coined the word *Tingis* and did not base it on the Greek word *Τίγγις*, and following the genitive used by Fabricius he makes the word an i-stem and writes the family name TINGIDAE.

Dr. Baker (1922, *Science* **56** : 603) contends that Fabricius introduced into the Latin language the Greek word *Τίγγις*, and since an i-stem in Greek made it an i-stem in Latin, following Article 4 strictly, he writes the family name TINGIIDAE.

Dr. Holland (1922, *Science* **56** : 535-6) replies to Dr. Parshley objecting to his stand.

Dr. Parshley (1922, *Science* **56** : 754) accepts Dr. Baker's conclusion about the origin of the word, but objects to the application of Article 4. He claims that Dr. Baker introduced this use in such cases.

Dr. Baker informs you of the fact that the word mentioned by Dr. Parshley, APHIIDAE, has been in the literature for ten years, so that others have followed Article 4 in such cases.

Shall Article 4 be followed ?

This was submitted to the International Committee on Entomological Nomenclature, which, at Paris in 1932, passed the following resolution :—

TINGIDAE *versus* TINGITIDAE *et* TINGIIDAE

Tingis étant un nom latin dont le génitif est *Tingis* et l'accusatif *Tingim*, TINGIDAE est la forme correcte de nom de la famille.

This resolution was confirmed by the International Commission on Zoological Nomenclature at Lisbon in 1935, and the *Opinion*, as above, published in 1943.

For our purpose the above citations from the *Opinion* will suffice, the remainder being merely the indication (at no little length) of the authority under which it is issued.

A first comment must be that it was nine years before Dr. Baker's case was considered at all, three more before the confirmation, and twenty in all before the *Opinion* upon it was published. It is certainly better to be slow than to be wrong, but this is a speed which, since zoologists must use *some* nomenclature, can only mean the establishment of wrong names during the period while the right ones are emerging.

The second comment is that the only defence of the *Opinion*, since it rejects Dr. Baker's view, is contained in the Resolution, and that, to an ordinary classical scholar, every single statement in that Resolution is wrong. For (*a*) the Latin word is not *Tingis* but *Tinge* or *Tingi* ; (*b*) its declension is

unknown ; (*c*) if its genitive were *Tingis* and its accusative *Tingim*, it would certainly be a third declension noun, i.e. an i-stem ; (*d*) and in that case the application of Article 4 would necessarily give TINGIIDAE as the family name.

Nevertheless, curiously enough, I think that the *Opinion* in favour of TINGIDAE is right, and that it can be adequately defended. But the consequences of that defence must be a more careful statement of Article 3 and a full interpretation (or a re-writing) of Article 4. If, as I suggest, we can treat *Opinion* 143 as case-law, both these things will become possible.

A short discussion of Dr. Baker's case, as quoted above, will clear the ground. A discussion of this kind ought, of course, to have been included in the *Opinion*. For full details I must refer readers to the five articles cited.

In the first place we note that Dr. Baker's summaries are not wholly accurate and are in one case, his own article, so ungrammatical as to be obscure at a crucial point. Further, in his summary of Dr. Parshley's second paper he completely omits matter which probably influenced the International Committee more than anything else in making its decision, since at this point he had taken the opinion of eminent classical scholars, though he had not, in fact, put the problem before them sufficiently fully. It is really their answer that is embodied in the Resolution and *Opinion*. But, as Dr. Baker urges, their answer does not conform to the requirements of Article 4. It may be noted that the International Committee makes no reply to Dr. Baker's question " Shall Article 4 be followed ? " and that they do not in fact follow it.

The basic fact is that Fabricius, 1803, *Syst. Rhyng.* : 124, erects the name *Tingis* and, in a footnote, uses the genitive *Tingis*. I can find no evidence for the alleged accusative *Tingim* cited in the Resolution. If it exists it would strengthen Dr. Baker's argument, as against the *Opinion*, to some small extent. But the genitive *Tingis* is sufficient in itself to establish the stem *Tingi-*, as opposed to *Tingid-* (cf. *Empid-*) or Holland's suggestion *Tingit-*. *Tingis* is clearly the Latinized Greek form of the North African (Berber ?) name given on coins as Tenga, Tinga, Tetga (Pauly-Wissowa, *Real-Encyclopädie*, **6** : 2517), and in the Latin geographers (Pliny, *Historia Naturalis* V. 1. 1 ; Pomponius Mela, I. 1. 4) as Tinge or Tingi. The place, the modern Tangiers, gave its name to the district known as *Tingitana* (Pliny, *loc. cit.*), but this does not, as Holland suggested, establish a stem *Tingit-*, any more than *Neapolitana* establishes a stem *Neapolit-* for *Neapolis*, which makes its genitive simply *Neapolis*. But Holland is right in saying that Fabricius is using the place-name Tingis in its Latinized Greek form and not just inventing a new Latin word, for Fabricius gave the preceding genus the name of *Syrtis*, the famous sandbank off the North African coast, not far from Tingi. The name in Greek is found in Strabo (I. 3. 1 (with genitive,-ιος), III, 1. 8 ; Ptolemaeus, IV, 1. 5 ; Dio Cassius, XLVIII, 45 ; Stephanus Byzantius, 655).

Thus Parshley's first contention is wrong, and Baker's main contention is right, though his further argument that an i-stem in Greek makes an i-stem in Latin, true up to a point, needs, as we shall see, much consideration.

Parshley's final contribution is more important, and Baker does not do it justice. It is true that Parshley accepts the evidence as to the origin of

the name, but he makes the important remark that " Fabricius did not adopt the word, rather he introduced into the neo-Latin language the word *Tingis*, genitive *Tingis*, stem *Tingi-*." This distinction of neo-Latin from Latin is of far-reaching importance in the whole discussion. He then goes on to point out that the classical mode of forming patronymics from i-stems always proceeded by the elision of the redundant i, " and indeed who ever heard of such terms as Apiidae, Aphiidae, Feliidae, or Caniidae." He then cites eminent classical authorities, Mr. H. Pennypacker and Prof. C. H. Moore, as follows :—

" Rules regarding the formation of family names which may be described as patronymics are subject to modification not only in the interest of convenience, but also of euphony, and in spite of the fact that the stems of the nouns you mention [*Nabis*, *Apis*, *Tingis*, *Coris*, *Aphis*] in Latin end in ' i,' and that the termination -idae is conventional in such cases there seems to be no doubt that the spelling with a single ' i ' carries universal authority and the penultimate ' i ' is short in quantity."

It is, of course, true that zoological family names are intended to be formed as patronymics, but they are, nevertheless, something else, scientific terms in " neo-Latin," with a precise purpose. Dr. Baker's case, to which the Resolution and *Opinion* make no reply, is that Article 4 precludes us from following the classical usage correctly stated in Dr. Parshley's article, and that we must stand by the letter of the law and not say, as Parshley does, that " the authors of the International Code, of course, were fully conversant with this principle, and expected it to be applied in connection with Article 4, as it had been in the past," since they were, in fact, " educated men, and their own works contain no such monstrosities as Feliidae or Anguiidae."

The *Opinion*, without intelligible argument, supports Dr. Parshley. But if it is correct, as I think it is, it is on grounds other than those stated or implied, and it involves the admission that Article 3 is not clear and that Article 4 completely fails to state its own intention.

Article 3 is the easier case. Where it fails is in its tacit assumption that classical Latin is the only Latin which need be taken into account and that all the complicated multitude of amorphous generic names can be " considered and treated as such." In the formation of family and sub-family names this is certainly not the case, as we shall see, and other difficulties arise of various kinds, especially in the establishment of genders. It is highly desirable that the International Commission should attach a note to the Article in the following terms :—

" It should be noted that Latin as used for scientific purposes, and especially as used at the end of the eighteenth century, is a living language not agreeing in all respects with the Latin commonly called classical. While classical Latin is necessarily the standard to which scientific terms should conform, that standard must not be so applied as to override considerations of scientific accuracy, uniformity, intelligibility, and practical usefulness."

The importance of some such note will appear in what follows.

Article 4 suffers from a radical defect. It does not define what is meant by the stem of a word. As a result we note, first, that it has never been applied at all in a large number of cases and, second, that it cannot be applied to many of them without absurd results. An example or two will make this immediately obvious. The stems of Latin nouns end with the five vowels of the five declensions, except in some cases, often derived from the Greek, where a consonant, implicit in the nominative, appears in the genitive. Thus the stem of *Plusia* is *Plusia-*, of *Gryllus Gryllo-*, of *Apis Api-*, of *Empis Empid-*. By applying Article 4 literally we get PLUSIAIDAE, GRYLLOIDAE, APIIDAE and EMPIDIDAE, the last alone being euphonious and reasonable.

To interpret the Article in this way would make hay of our nomenclature, and nobody has even attempted to do it, except in the case of certain i-stems. Words such as TINGIIDAE are obviously not too shapeless to use, and Dr. Baker and others have pressed us in that direction. But if we agree we have got to admit the dreadful PLUSIAIDAE and, worse still, CECIDOMYIAIDAE, ANTHOMYIAIDAE and their kind. I think that there will be general consent that that cannot be done.

That leaves two alternatives.

First, we can form our family and subfamily names exactly as though they were Latin patronymics of the classical period. That would give clear and easily pronounced forms. The rules are (1) that a-stems form *-adae*, e.g. PLUSIADAE, CECIDOMYIADAE ; (2) that other vowel stems form *-idae*, e.g. GRYLLIDAE, APIDAE, TINGIDAE ; (3) that stems ending in a consonant add *-idae*, e.g. EMPIDIDAE, DOLICHOPODIDAE. This is the principle which has in fact been more or less operative throughout the whole period during which zoologists have been struggling with this particular problem. It would have been perfectly clear if Article 4 had not introduced the grammatical term "stem," which is barely intelligible except to classical specialists, working on the theoretical hypotheses of grammar and philology. It is the principle presupposed in Parshley's argument, though he did not work it out to its conclusion, or realise that it would mean introducing forms in *-adae*. Some writers, such as Meyrick, e.g. 1927, *Insects of Samoa*, **3**, fasc. 2 (Microlepidoptera), with E. E. Austen's note on p. 76, have frankly abandoned Article 4, and used such orthodox classical forms as GELECHIADAE, PLUSIADAE. But this solution has one great difficulty. There are very many generic names, such as *Notodonta*, *Ephydra*, which do not fall readily into the classical scheme, being of modern and often of obscure origin. And it has also the practical disadvantage of destroying the present uniformity of usage whereby *-idae* is the universal mark of a family name.

It was probably a not very clearly thought out argument on these lines which underlay *Opinion* 143 and the Resolution cited above.

But, second, another alternative is possible, and, I believe, necessary. We can retain the general use of *-idae* and *-inae*, and also retain Article 4 in its present form, if we re-define the word "stem," using it not in the grammatical sense, with reference to classical Latin, but in a practical sense,

applicable to scientific Latin, the neo-Latin of Parshley's article. This could be secured by a note attached to the Article in the following terms :—

" For the purposes of Article 4 the term ' stem ' is to be taken to mean either the grammatical and classical stem or such part of it as will make wholly clear the relation between the generic name and the name of the family or subfamily, and will at the same time give the family or subfamily name the simplest and most euphonious form compatible with that relationship. The stem, in this sense, will normally be found by putting the generic name into the genitive case and then cutting off the termination, -ae, -i, -is, or -us, according to the ordinary rules of Latin declension."

Such a note would in practice be found perfectly clear and easily applied. It would give a consistent usage, and, which is important, one in all essentials in agreement with the usage most commonly employed today. Thus forms like *Plusia* and *Cecidomyia* would give PLUSIIDAE and CECIDOMYIIDAE ; *Gryllus*, GRYLLIDAE ; *Tingis* and *Anthocoris*, TINGIDAE and ANTHOCORIDAE ; *Empis*, EMPIDIDAE. And there need be no qualms about this on the part of classical scholars, since it is not with classical Latin, but with scientific or neo-Latin that we are dealing. Difficult forms like *Notodonta*, for which it is hardly possible to determine a genitive (or a gender) could be dealt with out of hand, giving NOTODONTIDAE, as at present.

In two cases I think that a formal *Opinion* would be desirable.

(1) *Cis*. Here the root is *Ci-*, the i being long. But CIIDAE is neither clear in its relation to *Cis* nor euphonious, and the obvious family name should be CISIDAE (Grensted, 1940, *Ent. mon. Mag.*, **76** : 145-6).

(2) *Aphis*. Here an *Opinion* should decide between APHIDAE and APHIDIDAE, either of which is equally tenable. Personally I prefer APHIDAE (Grensted, 1946, *Ent. mon. Mag.*, **82** : 246-7).

It is the great service of *Opinion* 143 that, treated as case-law, it can be regarded as a ruling which, coupled with Article 4, renders this general solution practically inevitable. I have no doubt that the notes suggested above embody the principles by which the International Committee, consciously or unconsciously, was guided, and I hope that *Opinions* on these lines may be promulgated as soon as possible. They would solve quite a number of our problems at a stroke.

DOCUMENT 3/7

By JESUS MOURE, C.M.F.
(*do Museu Paranaense e Universidade do Parana*)
and L. TRAVASSOS FILHO
(*do Departamento de Zoologia de Sao Paolo*)

Notas sobre a Nomenclatura dos Grupos Superiores a Generos

(Published in November 1947 in *Museu Paranaense*, Publicacoes Avulsas No. **4** : 1-19)

EDITORIAL NOTE.—The following document is the Summary annexed by the authors to their paper.

Summary

Basic principles to render uniform the nomenclature of groups above genera.*

(1) The suffixes that designate Super-families, Families, Sub-families, Tribes and Sub-tribes are, respectively : **oidea, idae, inae, ini, ina.**

(2) In order to add these suffixes to Latin (or Latinized) or Greek words, the rules of these languages must be obeyed, both for the investigation of the stems and for the combination of the vowels.

(3) Undeclinable nouns, or nouns belonging to other languages, and not Latinized, are to be considered as absolute stems. Suffixes must then be added without any alteration.

The second principle will here be more amply discussed, in order to render easier the task of the non-initiated in the Latin and Greek languages.

(a) Latin or Latinized Words

The following words are to be considered here :—

Latin (v.g. : **Musca**) ;

Greek, when used in Latin (v.g. : **Buprestis** < βούπρηστις) ;

Latinized Greek words (v.g. : **Leptocerus** < Λεπτο + κερας) ;

Latinized words of other languages (v.g. : **Knowiella** > *Know*).

After the stems are found, the designations of families, etc., are formed according to the following rules :—

(1) The stems of the 1st, 2nd, 4th and 5th Latin declensions are respectively a, o, u and e. The ending must be taken from the genitive, as found in dictionaries, the thematic vowel being then added. In the third declension the stems may end in consonants or vowels (i or u), and to form them it is only necessary to take the desinence from the genitive.

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*The authors are much indebted to Dr. F. Lane and R. L. Araujo for their help in the preparation of the English summary.

(2) The suffixes that designate families, sub-families, etc., eliminate the last thematic vowel (excepting u in the third declension), taking its place, or are added immediately to the stems ending in a consonant, or u, of the third declension.

Examples : *Musca* < Muscae < Musca < Musc < **Muscidae**

Culex < Culicis < Culic < **Culicidae**

Sus < Suis < Su < **Suidae**

(b) Greek Words

We consider as such the words that keep their genuine Greek desinence in the nominative, and not used in Latin.

The following rules, notwithstanding their being a little more involved, are easily applicable :—

(1st) The nouns belonging to the 1st and 2nd declensions have the stem a or o (α or ο), and drop this last vowel before receiving the suffixes **idae, inae,** etc.

Example : *Cecidomyia* < Cecidomyi < **Cecidomyiidae.**

(2nd) The stem of the words belonging to the 3rd declension are to be obtained from the genitive as found in dictionaries. The following remarks should be kept in mind :—

(a) Genitive ος

Preceded by consonant or ω it is sufficient to take off the desinence, in order to obtain the stem.

Example : **Schistosoma** < Schistosomatos < *Schistosomat.*

Preceded by α, ο or υ the ος desinence is taken off, and a digamma (F) is added in order that the stem may be obtained.

Example : *Echimys* < Echimyos < Echimy (F).

(b) Genitive ως

When not preceded by ε, is usually a contracted noun, or a jomic form. Dictionaries always give in first place the normal form, which should be used.

When preceded by ε, the following cases are to be considered, according to the nominatives :—

(1) Nominative υς, stem α (F).

Example : νεως (ναῦς)=na (F).

(2) Nominative ευς, ὺς or ὐ, stem ε (F).

Example : φονέως (φονεύς)=phone (F).

(3) Nominative ις or ι, stem ε.

Example : Μάντεως (μάντις)=Mante (j) (=Manti).

(c) Genitive ους

Nominative ες, ης or ος stem ε (σ).

Example : σαφούς (nom. σαφες) σαφε (σ)=saphe(s).

Nominative ως stem ο (σ).

Example : αιδούς (nom. ἀιδως) αιδο (σ)=aedo(s).

Nominative ω, stem ο (j).

Example : πειθούς (nom. πειθώ) πειθο (j)=pitho (j).

(3rd) After the stem is obtained, according to the second rule, the following rules should be obeyed :—

The stems ending in a consonant or in ω receive the suffixes without any alteration.

Example : **Phaenomeris** (φαινομερις) < Phaemonerid < **Phaenomeridae.**

The stems ending in α (F), ο (F) and υ (F) drop the digamma (F) and receive the suffix without any other alteration.

Example : **Echimys** ('Εκιμύς) < Echimyos < Echimy (F) < **Echimyidae.**

The stems ending in α (F), ε (σ), ο (σ), ε (j), ο (j) drop this assemblage, before receiving the suffixes.

Example : **Coelioxys** (Κοιλιοξυς) < Coelioxeos < Coelioxe (F) < **Coelioxinae.**

NOTE : The authors have very serious doubts with regard to certain generic names of Greek origin, of a definitely adjectival character, but with the incorrect ending is. These nouns are in existence in Greek, but have a different meaning as, for instance, **Eucnemis** ('Ευκνήμις) that should normally be **Eucnemos** ('Ευκνήμος). In accordance with Brues & Melander we prefer to interpret these irregular formations as Latin adjectives of the 3rd declension. Thence **Eucnemidae,** instead of **Eucnemididae,** because 'Ευκνήμις—ιδος means he who has good leggings, while **Eucnemos** (Εὔκνημος) means he who has good legs. The same applies to **Megalostomis** (Μεγαλοστομις, of irregular adjectival form), which should give origin to **Megalostominae** because Στομίς—ἰδος means the part of the horse-bit that is put into the horse's mouth. This meaning does not surely apply to the genus erected by Lacordaire for a group of the **Clytridae.**

DOCUMENT 3/8

Comment on Dr. Harold Manter's Application Relating to the Family Name Properly Applicable to the Genus " Dissotrema " Goto & Matsudaira, 1918 (Nematoda)

By the late LODOVICO DI CAPORIACCO
(*University of Parma, Italy*)

Extract from an enclosure to a letter dated 31st December 1947

As in the case raised by Dr. Manter,* the Family in question was first established for the genus *Dissotrema*, it is clear to me that that genus must remain the type genus of that Family. No one has the right to change it. If the name *Dissotrema* is an identical junior synonym of *Gyliauchen*—that

*See Document 3/3.

is, if the two genera have the same type species—then, of course, the name of the Family must be changed from DISSOTREMATIDAE to GYLIAUCHENIDAE. But the name of the Family ought not to be changed to OPISTHOLEBETIDAE, if the genus *Dissotrema* (or *Gyliauchen*) is placed in the same Family as *Opistholebes*, for that would involve changing the type genus of the Family.

DOCUMENT 3/9

Professor Jeannel's Proposal for the Introduction of a New Termination for Names Denoting Tribes

Note by the Secretary

Attention is drawn to a paper entitled " Sur la Nomenclature des groupements supergénériques " which was submitted to the International Commission on Zoological Nomenclature by Professor R. Jeannel in connection with its meeting held in Paris in July 1948. This paper has already been published in the *Bulletin* (1950, *Bull. zool. Nomencl.* **3** : 164-165), to which readers are referred.

It will be seen that the following points are made in the foregoing paper :—

(1) It is illogical to employ a feminine termination "—INAE " for the names of sub-families and the masculine termination "—INI " of the same word for the names of tribes. M. Jeannel therefore proposed that for the names of sub-families the feminine termination "—INAE " should be replaced by the masculine termination "—ITAE ".

(2) On the question of the priority to be assigned to the names of supergeneric groups, M. Jeannel expressed the following view :—

> La priorité appartient non pas au nom formé avec le radical du genre le plus anciennement décrit, mais bien au nom le plus anciennement proposé pour désigner le groupement, à la condition, bien entendu, que ce nom soit formé avec le radical de celui d'un genre faisant partie de groupement et actuellement valable. Contrairement au principe énoncé par Bradley (*Science* LXVII, 1928, p. 103), la priorité doit jouer pour les noms supergénériques, *même lorsqu'ils n'ont pas été donnés sous une forme latine plurale*, à la condition qu'ils aient été formés avec le radical du nom d'un genre contenu dans le groupement et actuellement valable. Le contraire eliminerait injustement l'oeuvre des grands entomologistes du début du XIX^{e} siècle, à qui on doit le fondement de la systématique des Insectes (par exemple, Lacordaire pour les Coléoptères).

(3) On the question of the rules to be laid down for the availability of names of supergeneric groups, M. Jeannel expressed the following view :—

> LA LÉGITIMITÉ.—Un nom de genre une fois donné est immuable. Il ne peut pas en être de même pour les noms de groupements supergénériques, qui n'ont rien d'obligatoire, car la constitution de ces groupements est conventionelle et variable.

. . . Le Code pourra dire, par exemple, qu'un nom de groupement supergénérique ayant la priorité pourra être rejeté comme " illégitime," lorsque son adoption entrainerait un désaccord évident entre la diagnose du premier description et celle du groupement tel qu'il doit être défini. Bien entendu, il entrera un élément subjectif dans l'application de ce principe de " légitimité ". Mais qu'on veuille bien remarquer que tout code doit être fait pour être appliqué par un tribunal jugeant selon l'esprit et non selon la lettre.

DOCUMENT 3/10

Note on the statement submitted in 1948 jointly by
Professor E. GORTON LINSLEY and Professor ROBERT L. USINGER

Editorial Note.—Attention is called, for purposes of record, to the fact that notice was given in 1948 of the proposed submission of a joint paper on the family name problem by Professor E. Gorton Linsley and Professor Robert L. Usinger, but that ultimately this paper was not submitted at the meeting of the International Commission on Zoological Nomenclature and of the International Congress of Zoology held in Paris in July of that year, owing to the decision that this whole problem should be subject to a detailed consultation with specialists before the next (Copenhagen) Congress in 1953 (1950, *Bull. zool. Nomencl.* **4** : 273). The above paper is not reproduced here because more recently a much more detailed statement of Professor Usinger's views has been received from Dr. W. I. Follett, Chairman of the Nomenclature Committee of the Society of Systematic Zoology. This later statement constitutes Appendix 1 to Dr. Follett's letter of 23rd July 1952 (Document 3/30, Appendix 1). Professor Linsley is a member of the American Committee on Entomological Nomenclature and, as such, took part in the preparation of the document submitted by that Committee (see Document 3/35).

DOCUMENT 3/11

By HOBART M. SMITH

(*University of Illinois, Department of Zoology, Urbana, Illinois, U.S.A.*)

Letter dated 12th June 1950

Enclosed herewith is a copy of a letter which brings up a point of procedure with regard to the formation of family names. Has any action on this point been requested already of the International Commission ? If not, there certainly is a need for it.

Enclosure to Professor Hobart M. Smith's Letter

Letter dated 12th June 1950 from Professor Hobart M. Smith to Dr. Ernest Williams, The Biological Laboratories, Harvard University, Cambridge, Massachusetts, U.S.A.

To perhaps carry an academic matter into the ground, I would like to pass on a viewpoint which at least merits consideration with regard to family names and the stems from which they were formed. Gradually I have been coming to the opinion that one should not form family names by use of stems obtained in the manner dictated by the language from which the name was derived. If the name happens to be obtained from some obscure language, obvious difficulties are courted thereby. Furthermore, in effecting the stem, one may duplicate another name, as for example was done in the case of *Emys* and *Emyda*.

I know that a large school advocates classical formation of stems for family endings, but in view of the certain confusion that this procedure occasions, I favour addition of the family name ending to a stem as nearly identical with the actual generic name as is phonetically possible. The International Commission has not apparently made a definite decision on this matter, although I hope that it will do so eventually.

DOCUMENT 3/12

Views of the late K. W. DAMMERMAN

(*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*)

Attention is drawn to the paper entitled " Proposals concerning the nomenclature of family names and of names of economically important insects " submitted to the International Commission on Zoological Nomenclature by the late Dr. K. W. DAMMERMAN (through Professor H. Boschma) in August 1951.

The above paper was prepared for consideration in the first instance by the Ninth International Congress of Entomology at its meeting held at Amsterdam in August 1951.

In view of the fact that the proposals in the foregoing paper have as their main object the promotion of stability in zoological nomenclature, it has been published as DOCUMENT 1/14 in the section of the present volume (pp. 21-22) concerned with the stability problem.

DOCUMENT 3/13

Information furnished by LEONARD P. SCHULTZ
(*Smithsonian Institution, Washington, D.C., U.S.A.*)

Attention is drawn to the paper enclosed by Dr. Leonard P. Schultz under cover of a letter dated 26th September 1951 (which is reproduced in the Ordinal Names series*), since, although it is concerned mainly with the problem of Ordinal Names, it deals also with the problems involved in the naming of Tribes and Superfamilies.

DOCUMENT 3/14

By P. C. SYLVESTER-BRADLEY, B.Sc.
(*University of Sheffield, Department of Geology, Sheffield*)

Statement dated 18th March 1952

The Terminations to be Adopted in the Formation of Superfamily Names

The terminations " -ACEA " and " -ACEAE " have been widely used in the formation of superfamily names of some groups. In the Ostracoda the former termination is in current use among palaeontologists and in the *Zoological Record*, and to change it to " -OIDEA " would lead only to instability of nomenclature. I would make the following proposals :—

(1) Certain cited alternative terminations should be regarded as permissible in the formation of superfamily names.

(2) It should be prescribed that all superfamilies of one Order must be terminated in the same way.

(3) The choice of termination for the superfamilies of a particular Order should be restricted by a ruling of priority.

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*See Document 4/6.

DOCUMENT 3/15

By L. B. HOLTHUIS

(*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*)

Letter dated 15th April 1952

I agree with the larger part of the suggestions in your paper (1952, *Bull. zool. Nomencl.* **7** : 61-94) concerning the proposed clarification, amendment and expansion of the provisions in the *Règles* relating to the formation of family names. On two points, however, I should like to make some remarks.

(1) Your paragraph **15**, p. 74. I do not quite agree with your suggestion put forward in this paragraph. To me it seems highly desirable that when the name of the type genus of a family is found to be a subjective junior synonym of an older generic name and thus has to be changed, the family name should automatically be changed also. In the example given by you the family name A-idae in my opinion should be automatically replaced by the name " D-IDAE." In my opinion, it is extremely unpleasant that the generic name on which a family name is based is not used for any genus in that family. Furthermore it will be more easy for zoologists not to have different rules in this case for objective and subjective synonyms. In the very rare cases that the rejection of the invalid family name would cause much confusion, a suspension by the Commission could be asked. As I have already stated before, it seems to me of the highest importance to keep the *Règles* as simple as possible.

(2) Your paragraph **18**, p. 77. I do not quite understand your clause that the status of competing family names be determined by reference to the relative dates on which the various type species concerned severally became the type species of the type genera of the families in question. My problem is best illustrated by the following example :—

(1) In 1830 a family " A-IDAE " is erected with the type genus *A-us*. (2) In 1850 the generic name *A-us* is found to be invalid and is replaced by the subjective synonym *B-us*. (3) In 1880 the family name " B-IDAE " is proposed to replace " C-IDAE." (4) In 1870, however, the genus *C-us*, which generally is considered to belong in the family " A-IDAE," was made the type of the family " C-IDAE." Your suggestion is, I believe, that the generic family name " B-IDAE," though it actually is junior to " C-IDAE," should take precedence over the latter name, since it has been proposed to replace the family name " A-IDAE," which is older than " C-IDAE." Or in other words that family names are to be regarded to date from the moment that their type genus (as fixed by its type species) became the type of a family, regardless of the name of the family and the genus at that time. I cannot see the importance of the date on which the type species of the type genus of the family in question became the type species of that genus. Should this mean that if the type species for the genus *C-us* was selected in 1870, while that of *A-us* and *B-us* was not selected before 1875, that the family name " C-IDAE " should take precedence over " B-IDAE " ? I know that this cannot be your meaning and that I thus incorrectly interpret your words, but I am afraid that many others, especially those who do not live in an English-speaking country, might have the same trouble.

Furthermore I wonder whether a strict application of the priority rule here would not be more practical than the rather complicated method discussed above, the more so since the Commission can use its plenary powers if the changes will cause too much confusion.

DOCUMENT 3/16

By D. K. McE. KEVAN

(*School of Agriculture, Zoology Section, University of Nottingham. Sutton Bonington, Loughborough, England*)

Enclosure to a letter dated 29th April 1952

Comments on the Problems Involved in the Regulation of the Names of Families Discussed in Part 3 of Volume 7 of the "Bulletin of Zoological Nomenclature"

Para. **7**

It is my opinion that the formation of family names should be liable for correction until such time as they actually appear on an *Official List of Family Names*.

Para. **8**

I am not especially keen on the stabilisation of incorrectly formed family names merely because they have not been corrected prior to a given date. The lack of correction may not be due to a lack of desire for such a correction but solely to neglect of the group in question and the lack of a reviser. It is possible that full family names may mostly have been used sufficiently frequently to determine this, but the position in regard to groups below the rank of family may be more obscure. I therefore repeat what I have said in my comment on para. 7.

Para. **11**

In my opinion, the author of a family name should be free to choose any genus as the type of the family. The type genus should, however, have a designated type species (which could be designated there and then), unless that genus was monotypic at the time of its description. It is suggested that a family name should remain technically invalid until such time as the type species of its type genus has been designated. When this has been done, the family name would automatically be validated as from the date of its original proposal. The reason for this suggestion is to safeguard against any ambiguity arising in the event of the type genus being divided into two or more genera. The provision would only come into force with the new *Règles*, but specialists should be urged to designate type species for genera wherever this has not already been done, especially for the type genera of families, etc.

In choosing the type genus of a family, the oldest included generic name should be selected unless there is good reason to avoid this, but this should be in the form of a " Recommendation " and not a Rule.

A family name should not be valid if the family concerned contains the type genus of an earlier described family. If, however, the two type genera are subsequently separated into different families, it should be possible to use the junior name again for the new family, dating from its original usage. If the junior generic name were transferred to another established family, the family name to be used for that family would depend upon the relative priority of the respective family names. Where a condition of instability might arise, application could be made to the Commission.

Para. **12**

A family name should date from the first use of the word " family " or " subfamily " or its equivalent (even if this word is not of Latin derivation) *or* it should date from the first use of the suffix -id-, -in-, or -it- (whether Latinized or not, e.g. -idae, -inae, -ini, -inos, -iden, -ites). The words " tribe," " sub-tribe," etc., and in fact any other than those specified above, in the absence of the suggested terminations, shall not be deemed valid in the designation of family names except by application to the Commission. I do not favour an arbitrary date before which almost any word signifying a supra-generic unit would be considered as a valid family name and on no account would I support a mere nominative plural of a generic name as having any status as a family name whatever.

Para. **14**

I am in agreement with this. I would suggest that the name and date of the author of the family in its initial form should be given in brackets as the author of the family name in its new form and that the later author who changed the name to its new form and the date should follow this without brackets—or some similar device to indicate how it might be that a junior family name derived, perhaps from a junior genus, may have priority over an apparently senior family name derived from a senior generic name.

Para. **15**

I may be against stability in nomenclature here but I am of the opinion that family names *must* change with any change in the name of the type genus. Family names can only be based on valid generic names. If for *any* reason, the name of the type genus is shown to be invalid, the family name becomes automatically invalid. With improved taxonomy (i.e. subjective treatment) difficulties in the way of stability will gradually be eliminated. Where undue confusion is likely to occur, there is always recourse to the Commission.

Paras. **16–19**

The family name of united families should be that of the oldest valid *family* name (as in 12 above). It should not be determined by the first reviser. The reviser, however should be at liberty (as any other person) to apply to the Commission if it would be confusing in the extreme to follow this course.

Paras. **21–24**

In this instance a change is necessary in my opinion. I do not support any idea that a family name should be allowed to stand for any reason whatever unless it be based on the valid name of the type genus. Orderly nomenclature has a comparatively short history and the fact that certain people do not wish to subscribe to the Rules now because of a certain amount of confusion which may arise for a year or two is no reason for making exceptions which will appear ludicrous in a century from now. I am wholeheartedly against the suggestion put forward in *Bull. zool. Nomencl.* **2** : 152. It is no more difficult nor confusing to get used to Meigen's 1800 names for the Diptera and to base the family names upon these genera than it has been to get used to the current usage of the family names ACRIDIDAE and TETTIGONIIDAE. Personally I am now quite used to such names as MELUSINIDAE and ITONIIDAE for SIMULIIDAE and CECIDOMYIIDAE. Why change it all back just as some at least are getting familiar with the change ?

Para. **25**—see Para. 14

Para. **26**

I should prefer RIODINIDAE (Swainston, 1827) Grote, 1895 since this seems nearer generic usage. When given in full this would be RIODINIDAE (ERYCINIDAE Swainston, 1827), emend. Grote, 1895.

Para. **31–33**—Fully agreed.

Para. **35**

I can see no reason for the recognition of super-super-tribes, especially if they are to be treated exactly as subfamilies. One can hair-split indefinitely and this category is inconsistent.

Para. **36**

Unless " tribes " etc. have a suffix of the form mentioned under para. 12 I am very dubious about their being co-ordinated with families etc., at least in respect of priority. It is my opinion that such names should have no priority status and to gain this they should first be published with an appropriate termination. They would then be valid as from that author and date.

It is my opinion that super-tribes, and sub-tribes should be given standardised terminations analagous with the termination "—INI " for tribes. They should however, be distinctive, as for example "—IDI " and "—ITI ". Such terminations would be purely arbitrary (if necessary) and not necessarily correct etymologically since it has already been pointed out that there are objections even to the accepted termination " —INAE " for subfamilies.

Groups of genera such as " tribes " named originally by merely using the nominative plural of the type genus, or by similar means such as adding the termination "—es " to the stem of the type genus, unless used with the word " family " (etc. —see Para. 12) should, irrespective of the status given by the original author (or any subsequent author), be regarded as of sub-tribal status. They would have to " work their way up " by being published with the agreed sub-tribal (or higher category) termination before they could ever be considered in any question involving priority. They would date as from the first usage with the approved termination. Sub-tribes not having the approved termination would obey the rules of priority among themselves but would not take priority over correctly formed tribal names.

Para. **40**

Agreed. Provision should also be made to reserve the termination " —OIDEA " for superfamilies and to prevent its usage for higher categories. It has been used (and still is in some cases) for sub-orders of " Helminthes " and for super-orders of Insecta.

Paras. **41–42**—Agreed.

Para. **43**—I fully endorse the need for an *Official List of Family Names.*

Para. **45**

The name of a family should under no circumstance be placed on an *Official List of Family Names* unless the name of the type genus has previously been or is simultaneously placed on the *Official List of Generic Names.* This will prevent any likelihood of the name of a type genus being changed and leaving a family name on the *Official List* which is derived from a stem differing from that of the " new " name of the type genus.

Para. **46**—Agreed in the main but see 36 above.

Para. **47**

I should prefer to see the word " Tribal " also included in the title.

DOCUMENT 3/17

Suggestions furnished by Dr. FRANZ DANIEL

(*Zoologische Sammlung des Bayerischen Staates, München, Germany*)

Editorial Note.—Attention is drawn to the letter dated 30th April 1952 from Dr. Franz Daniel (*Zoologische Sammlung des Bayerischen Staates. München*), which, being mainly concerned with the problem of promoting stability in zoological nomenclature, has been included in the series of papers which has been assembled in regard to that subject. It is there included as Document 1/25.

In the letter referred to above, Dr. Daniel suggested that the stabilisation of nomenclature should be sought by the establishment of committees of interested specialists in particular groups at the family level, wide powers being given to these committees to promote stability without being unduly trammelled by the principle of priority. It was an important feature of this plan that family names should be stabilised in harmony with established practice.

DOCUMENT 3/18

By Th. HALTENORTH
(*Museum, München, Germany*)

Statement, dated 14th May 1952, communicated by Professor E. M. Hering

II. Einverstanden. Bei B.2 und B.8 ist Fassung (*a*) zu bevorzugen.

Explanatory Note

The foregoing comment was prepared by Dr. Haltenorth in response to an invitation by Professor E. M. Hering who had prepared the following synopsis of the problems at issue for consideration by German zoologists. It is to this document that the numbers cited by Dr. Haltenorth refer.

II. Erweiterung der "Regeln" Bezuglich der Familien und Untergeordneter Kategorien Oberhalb der Gattung

A. Problem: Wie soll die Familien-pp-Benennung geregelt werden? Der Kongress von Graz (1910) änderte die Regeln dahin ab, dass nicht die "Wurzel," sondern der "Stamm" der typischen Gattung durch Anhängung von -idae etc. zum Familien-pp. Namen wird.-Paris 1948 entschied, dass unter "Stamm" der grammtikalische oder klassische Stamm des Wortes oder ein Teil des Stammes zur Familien-pp. Bezeichnung verwendet wird. In beiden Anderungen keine genügende Präzision.

B. Vorschläge für Neufassung: Zu den "Regeln" sollen "Ratschläge" gegeben werden die zur richtigen Stammfindung griechischer oder lateinischer Wörter anleiten.

(2) *Bei inkorrekten Bildungen*: Prinzipiell keine Emendierungen:

(*a*) Wenn die Bildung inkorrekt, sollen Spezialisten Antrag bei I.N.K. stellen, die nach Veröffentlichung etc. Emendierung vornimmt, weniger unter Berücksichtigung philologischer Gesichtspunkte als zweckmässiger Stabilität.

(*b*) Wo vor Neufassung der "Regeln" Emendierung erfolgt ist, die z.T. eingebürgert ist, soll die I.N.K. nach gebräuchlichm Verfahren entscheiden, ob und welche Emendation anerkannt wird.

(3) *Bildung* von Familien-pp. Namen *nach Neufassung* der Regeln: Die im einzufügenden "Ratschlag" gegebenen Richtlinien sollen bindend sein. Wo trotzdem inkorrekte Bildung, erfolgt automatische Emendierung.

(4) Welches *Genus* soll *Typus* der Familie sein? 2 Möglichkeiten:

(*a*) Recht des orsten Auswählenden (durch *Opinion* 141-Lissabon 1935 und Paris 1948 in die "Regeln" eingefügt).

(*b*) Älteste Gattung der Familie (bedenklich, da durch Auffnidung älterer hineingehöriger Gattungen Familienname geändert werden müsste!)

(5) Soll das *Datum in Prioritätsfragen* sein :

(*a*) Datum der ersten Familiennamenbildung ohne Rücksicht auf Endung ? oder

(*b*) Erste Familiennamen-Bildung mit -idae ? Bei anderen Kategorien ähnlich. Vorschlag : bis etwa 1850 sollen Bildungen auch ohne -idae pp. gültig sein, später nur mit -idae pp.

(6) *Priorität*, wenn der älteste *Gattungsname ersetzt* werden muss. Vorschlag : Bei Anderung des Namens der typischen Gatung Datum der Bildung durch diese Gattung, wenn sie von einem Artnamen begleitet war.

(7) Familien-pp. Bezeichnung wird *nicht geändert*, wenn ein Autor subjektiv die *typische Gattung als identisch mit einer anderen*, älteren ansieht. Wo aber Spezialisten den Antrag stellen, die Anderung vorzunehmen, soll die INK. nach vorgeschriebenem Verfahren eine Entscheidung herbeiführen.

(8) Bei der *Vereinigung zweier Familien* Name der neuen Familie :

(*a*) Nach der Familie mit dem altesten typischen Genus zu wählen ? oder

(*b*) Das Recht der Auswahl dem 1. Bearbeiterzu geben ?

(9) Im Interesse der *Stabilität* soll bei Vereinigung die neue Familie den *länger bekannten* bezw. auf das am *meisten verbreitete Genus* gestützten Namen tragen, gegebenenfalls auf Antrag bei der INK.

(10) *Ausnahmen* von der Regel, dass in jedem Fall die Familie pp. nach dem typischen Gattungsnamen gebildet wird :

(*a*) Einige *eingebürgerte* Familiennamen haben *keine gleichnamige* typische Gattung.

(*b*) Es kann manchmal *bei Änderung des Gattunganamens Beibehaltung* des Familiennamens wünschenswert sein (Chironomidae-Tendipedidae). In beiden Fällen Entscheidung durch INK. auf Antrag der Spezialisten.

(11) Bei Einführung der Priorität für *Familiennamen* sollen diese entsprechend den Gattungsnamen *Autor und Datum* bei der Schreibung erhalten.

(12) Bei *Homonymie* ist der jüngere Name zu verwerfen ; geringfügige Unterschie de rechtfertigen Selbständigkeit. Bei Homonymie auf Grund verschiedener Gattungbezeichnungen (z.B. Cyprinidae bei Pisc. von Cyprinus, bei Moll. von Cyprina) entscheidet auf Antrag die INK.

(13) Die Regeln für die Familien gelten auch für Unterfamilien (-inae) und Tribus (-ini), obgleich die Endung der Tribus sprachlich ein Neutrum zugrunde legt.

(14) Die *nominotypische* Unterfamilie soll -abgesehen von der Endung -der Familie gleichen, gleiches entsprechend für Tribus.

(15) Die *Kategorien* Familie und die unterhalb von ihr stehenden über der Gattung stehenden sollchen in gleicher Weise *koordiniert* sein, wie es Gattung und Untergattung sind.

(16) Weitere zu schaffendende untergeordnete Kategorien sind in gleicher Weise mit den vorgenannten koordiniert.

(17) Gleiche Regeln sollen auch für das *Supergenus* gelten, doch sollen diese *Priorität nur unter sich* haben. Welche Endung wird vorgeschlagen ?

(18) Für *Superfamilien* gelten gleiche Verfahrenvorschriften, doch auch diese sollen *Priorität nur unter sich* haben. Endung : -oidea.

(19) Vorschlag auf Schaffung einer " Offiziellen Liste der Namen der Familien und untergeordneter Kategorien " (" Nomina conservanda ") und eines " Offiziellen Index der verworfenen und ungültigen Namen für Familien etc." Beide Listen analog den schon bestehenden für Gattungen und Arten, mit den gleichen Verfahrensvorschriften für die INK.

(20) Fragen an= die Spezialisten :

(*a*) Besteht Übereinstimmung mit den Vorschlägen ?

(*b*) Wenn nicht, was soll geändert werden ?

(*c*) Welche Gesichtspunkte sind nicht berücksichtigt, Vorschläge für diese ?

DOCUMENT 3/19

By W. J. ARKELL, M.A., D.Sc., F.R.S. (*Sedgwick Museum, Cambridge University, Cambridge*)

Letter dated 23rd May 1952, with enclosure

I have read with admiration your most thorough and clear papers in the *Bull. zool. Nomencl.*, vol. **7**, Parts 1–6.

I enclose comments on Z.N.(S.)357 (family names). I should be content to see . . . everything . . . not covered by my comments go through as expounded in your papers.

Enclosure to Dr. Arkell's Letter of 23rd May 1952

P. 89, *Para.* **40**

Terminations for superfamily names. In Mollusca "—ACEA ", "—ACEAE " have been generally used. Of these "—ACEAE " was recommended to authors of sections of the *Treatise on Invertebrate Paleontology* for all fossil invertebrates, but as some complaints were raised, the recommendation was withdrawn in favour of private enterprise. I have always used "—ACEAE " like most previous workers on Cephalopods. The ending "—OIDEA " is hopeless for Cephalopods, being that used for the Orders Ammonoidea, Nautiloidea, etc.

Para. **41**

The last sentence (p. 90) seems to me essential.

DOCUMENT 3/20

By LESLIE BAIRSTOW, M.A.
(*British Museum (Natural History), Department of Geology, London*)

(1) Letter dated 26th May 1952

After studying your paper in Vol. 7 of the *Bulletin of Zoological Nomenclature* on family names and related matters, I now send my comments on the questions which you raise.

The proposals that you have put forward seem to me to constitute a well-thought-out and coherent scheme, and I find myself in general agreement with much of what you have written.

At first sight the number of different circumstances in which special application to the International Commission may become necessary is a little disturbing. However, these are, of course, problems that are not amenable to hard and fast rules, and I suppose that it is well to state explicitly particular circumstances in which applications to the Commission are called for. As such circumstances relating to family names are all exceptional, in practice the combined burden from this source may prove after all not to be unduly heavy.

Paras. **4-10**

As regards the problem, discussed in paragraph 4, of defining "stem," I consider that the particular solution set out in *Bull. zool. Nomencl.* **4** : 246 involves an undesirably large subjective element. I am glad to note that this view is shared.

From paragraphs 4 to 6, I conclude that throughout paragraphs 4 to 10 the intention is to deal solely with the problems peculiar to determination from correctly-spelt generic name of its stem on which a family name is to be formed—as distinct from the problems of orthography that concern equally the generic name, its stem, and any family name formed from its stem. On this assumption I agree that the original form of the stem of a family name published prior to the coming into operation of the revised scheme should not be emended except by the Commission, but that emendations already in common use should not be discarded pending decision by the Commission ; I agree also that formation of family names published after the coming into operation of the revised scheme should be governed by the rules and Schedule suggested.

Paragraphs 8 and 9 seem however to be so worded that they might apply not merely to emendations relating to the construction of a family name from a correctly-formed generic name, but also to emendations of family names that may arise from problems of orthography that concern equally the generic name, its stem, and the family name formed from its stem. Thus it seems that there might be some overlap between the provisions here suggested, according to the present wording, and the general provisions for emendation of zoological names that are suggested in 1952 *Bull. zool. Nomencl.* **7,** Parts 1/2. Perhaps this is inadvertent, for it seems to me that according to the proposals

as they are at present worded, certain automatic emendations of an incorrectly-formed generic name are possible, but that if a family name had been formed from the stem of the still-unemended generic name, the comparable emendation of the family name could not be validly effected without reference to the Commission : whereas an emendation of the family name in sympathy with an emendation of the generic name should preferably be automatic.

Paras. **11** *and* **17**

I strongly support continuation of the present rule that an author establishing a new family should be free to select whatever genus he considers the most appropriate to be type genus.

Paras. **12** *and* **13**

I agree that the essence of the general rule should be " that a name is to be treated as having been published as a family name only when the term used for this purpose was formed with the termination ' -IDAE '." It should be made clear whether, conversely, a name that was proposed with the termination " -IDAE " should be automatically available as a family name even if not proposed as a family name, so long as originally of supra-generic rank. I agree with Richter (1948, *Einführung in die Zoologische Nomenklatur*, 82-3) and R. C. Moore (in the unpublished *Treatise* Circular 4, note 9) that such a name should be automatically available ; and certainly the use of such a name in any other sense would be contrary to the Commission's recommendation (1950 *Bull. zool. Nomencl.* **4** : 261).

In confining the above statement of the essence of the general rule to family names and the termination " -IDAE," I assume, of course, that other categories of the family group of categories, and their appropriate terminations, will be brought under the rule by such provisions as may be accepted in relation to paragraphs 30 to 42. It should be made clear, in addition, what is to be the application of the general rule to a case such as that of the Family OMMASTREPHINI Steenstrup, 1861, where the termination is a permissible termination for a category of the family group of categories, and yet is not appropriate to the particular category to which the name concerned was assigned by its author. I think that, in this example Steenstrup should be considered to have made potentially available, as from the date of his publication, OMMASTREPHIDAE, OMMASTREPHINAE, OMMASTREPHINI, and so on—i.e. all forms of the name appropriate to the various categories of the family group of categories.

I agree that the general rule should be supplemented by provision for exceptions, to allow of retrospective attribution, at any rate in certain instances in which the publication of some unorthodox form of a family name, at an earlier date than its publication in orthodox form, may nevertheless be held to constitute effective publication of the family name. I agree also that no exception should be permissible in favour of any improperly-formed family name published later than an agreed limiting date such as 1850.

As you will have gathered, I hoped at one time that it might be possible to secure agreement on rules defining the limits of possible exceptions of various

kinds in such detail that it would rarely be necessary to submit individual exceptions to the Commission.

However, though I am one of those zoologists who are reluctant to accept non-Latinized family names as having any status at all, there would evidently be difficulty in securing agreement on a rigid rule even relating to this fundamental question, if it is indeed the case that in some groups family names are commonly accepted as dating from original publication in non-Latinized form. Exceptions in favour of non-Latinized names should certainly not be permissible without the express sanction of the Commission in each individual instance ; and if such exceptions are to be allowed, Articles 3 and 21 should be modified in such a way as to make it clear that these Articles are not thereby infringed.

Probably there would be equal difficulty in securing agreement on a rule that there should be no exception in favour of family names that are Latinized but are not plural in form (Example : Family BELEMNOSEPIA Buckland, 1836) ; especially as it is not always clear whether a name is singular or plural in number.

It seems, moreover, a reasonable claim that no exception should be permissible unless it had secured acceptance, by workers on the group concerned, before the coming into force of the new rule ; and this is a policy to which it would indeed be difficult to give effect without referring each proposed exception to the Commission.

I am therefore ready to accept that there should be no exception to the general rule, " save that, in the case of families which prior to the introduction of the revised scheme have been treated as having been established by the publication before 1850 of a term not bearing the foregoing termination, it shall be open to the International Commission " . . . " to direct that the family is to be treated as having been established as from the date of the publication of a term not bearing the termination ' -IDAE.' "

I agree that, as suggested, there should be provisions regarding public notice and the criteria to be followed by the Commission, also a " Recommendation " deprecating hasty name-changing.

Para. **14**

I agree that a family under its changed name should retain its original priority when, though the name of its type genus is changed, the type species of the type genus remains unchanged.

Para. **15**

I agree that when the nominal genus upon which a family name is based is subjectively identified with another nominal genus having an older available name, the family name should not be subject to change except by the Commission in response to consensus of specialist opinion.

Para. **16**

I agree that, in deciding between competing names for a family, the criterion should not be possession of the type genus having the oldest name of any of the names of the type genera concerned.

Para. **17**

See paragraph 11.

Paras. **18-20**

I favour basing decisions between competing names for a family on priority as between the dates at which the various type species concerned severally came to be the type species of the type genera of the families in question; subject to a proviso that, where specialists represent to the Commission that difference of opinion is liable to lead to the sinking, as a subjective synonym, of a well-known family name in favour of a little-known family name, it shall be open to the Commission in response to consensus of specialist opinion to direct which of the family names concerned is to take precedence over the other.

Paras. **22-24**

The following are examples of family names that are not based on generic names :—

(1) POLYTEUTHIDAE Stolley, 1919.

There is no genus *Polyteuthis*.

(2) HASTATIDAE Stolley, 1919.

There is no generic name with the same stem. The derivation is presumably from Hastati (one of the sections into which the broad genus *Belemnites* was formerly divided) and from the species *Hibolites hastatus*, rather than from the genus *Hastites* Meyer-Eymar, which in its restricted sense is quite outside the HASTATIDAE.

In other examples the family name is at any rate not formed simply by the addition of " -IDAE " to the stem of the generic name :—

(1) PROTOBELEMNITIDAE A. P. Pavlow, 1913.

There is no genus *Protobelemnites*.

(2) EUBELEMNITIDAE A. P. Pavlow, 1913.

There is no genus *Eubelemnites*.

(3) NEOBELEMNITIDAE A. P. Pavlow, 1913.

There is no genus *Neobelemnites*.

Presumably these three names are based in a sense, though I think not in a relevant sense, on the old broad genus *Belemnites*.

I agree that a family name not based directly upon the stem of the objectively valid name of its type genus should not be validated by the Commission except as a last resort. i.e. in the circumstances that the family name is an early one and so well established as the name for a concept that is still currently required that its preservation is considered essential. also that the end cannot be more readily achieved by validating the generic name (if any) on which the family name was based.

As regards availability for such validation. each of the family names that I have quoted above as examples would be disqualified two or three times over.

I take it that the subsequent proposal of a generic name with the same stem, even if explicitly based on that of the invalid family name, would not suffice to validate the family name retrospectively.

I assume also that if a generic name is suppressed under the plenary powers, any family name formed from the stem of this generic name falls with it unless the Commission makes special provision for the exceptional retention of the family names.

Para. **25**

I agree that author and date of family name should relate to first publication of the name in its accepted form, subject to certain exceptions. Exceptions should not be restricted to the special case of a substitute family name discussed in paragraph 14, but should include also, in particular, any special case arising from paragraphs 12-13.

Para. **26**

I agree that normally the author and date of a family name should be cited after the same style as the author and date of a generic name.

There remains the question of style to be adopted in instances of retrospective attribution of family names. The suggested style exemplified by "RIODINIDAE (ERYCINIDAE emend. Grote, 1895) Swainson, 1827" may tend to be a little misleading, for it seems to link unduly the author Grote with the name ERYCINIDAE and the author Swainson with the name RIODINIDAE, a reversal of the primitive relationship.

Richter (1948, *Einführung in die Zool. Nomenklatur*, 88) suggested a different style of citation. As applied to the same example, his system would yield "RIODINIDAE (Swainson, 1827) Grote, 1895." which might be expanded as "RIODINIDAE ERYCINIDAE (Swainson, 1827) Grote, 1895." This, however, seems to me to give undue prominence to Grote, 1895, when for purposes of priority of RIODINIDAE the significant date is 1827.

Naef (1922, *Die fossilen Tintenfische*, 297) employed "LOLIGINIDAE Steenstrup, 1861 (as LOLIGINEI)," equivalent in our example to "RIODINIDAE Swainson, 1827 (as ERYCINIDAE)." I suggest the adoption of this style for general use, and its expansion, when fuller history is required, to the style of "RIODINIDAE (Grote, 1895) Swainson, 1827 (as ERYCINIDAE)."

Whatever style is agreed on should be applicable whenever there is retrospective attribution, whether in the circumstances of paragraph 14 or of paragraphs 12-13.

Paras. **27** *and* **28**

I agree that there should be provision for eliminating homonymy that arises when names of different families are based on generic names that are different yet have the same stem. I agree, further, that, except when good reason to the contrary is demonstrated, the homonym relating to the earlier-established family should be regarded as the senior homonym to be retained unchanged.

It is apparent from *Opinion* 140 that it is possible for the Commission, at any rate in some cases, to devise a small change sufficient to eliminate trouble some homonymy without seriously disturbing the relationship between the name of a family and that of its type genus. Moreover, in view of the importance of preserving this relationship (as stressed in paragraphs 22-24) I assume that any total rejection, rather than trifling emendation, of a homonymous family name would involve changing not only to a different family name but also to a different nominal genus as type genus. If the change to a different genus as type genus were purely a nominal change, the generic name adopted still relating to the same genus, the change would involve use of the Commission's plenary powers ; whilst a change to a genus considered different though con-familial would commonly involve establishing a new family name—in which event the non-availability, as type genus, of the genus whose name would lead to homonymy at family level, would conflict to some extent with the desideratum (paragraph 11) that an author establishing a new family should be free to select whatever genus he considered the most appropriate to be the type genus.

Because of these considerations I doubt the desirability of including any provision for automatic rejection of family names that are junior homonyms. Especially as homonymy of family names is only of rare occurrence, I favour endorsement of the interim decision that where the application of Article 4 led to the establishment of two or more families having the same name, the case is to be referred to the Commission.

Para. **29**

I agree that rules for determining whether generic names are to be treated as homonyms should apply also to family names.

Para. **30**

I agree that the revised scheme should contain an express provision that except in so far as there is express provision to the contrary, the naming of sub-families shall be subject to the same provisions as those governing the naming of families.

Para. **31**

It is unfortunate that the terminations " -INAE " and " -INI " apparently differ etymologically only in gender ; and that the termination " -INAE " is claimed to be objectionable to French zoologists because of its feminine gender.* But generic names are not all of masculine gender, nor are names of higher categories !—and in any event the termination " -INAE " for sub-families is so well established that I should prefer to accept its defects rather than to invite the confusion that would be likely to result from attempting to enforce a change.

*See Document D3/9.

Para. **32**

I agree that there should be explicit provision that the subfamily containing the type genus of the family should have the same genus as its type genus and should be known as the nominotypical subfamily; and that its name should differ from that of the family only by the appropriate differences of termination.

Para. **33**

I strongly favour treating family and subfamily names as co-ordinate.

Paras. **35-38**

Here it seems to be taken for granted that the expression " tribe " should be used in the sense of a category between subfamily and genus. I do not think that this should be assumed without discussion, for the expression " tribe " has often been used in other senses, and especially for a category between suborder and family.

According to W. T. Calman (1949, *The Classifications of Animals*, 18) " intermediate groups with names like . . . Tribe . . . have been used indiscriminately for categories of the most diverse values."

Tribe has been used at a level between suborder and family by, for example, Sedgwick (1898-1909, *A Student's Text-Book of Zoology*) in Crustacea, Gastropoda, Pisces, Reptilia and Aves, by Bülow-Trummer (1920, in *Fossilium Catalogus*) in Dibranchia, and by Grimpe (1922) in his important classification of Cephalopoda.

As Calman made clear (*loc. cit.*, pp. 21-22), the level at which optional extra categories are needed varies in different parts of the Animal Kingdom. In Dibranchia the need is for extra categories between suborder and family rather than between subfamily and genus. If consensus of opinion indeed favours restricting tribe to the main category between subfamily and genus, some other term should be made available for the main category between suborder and superfamily.

Para. **35**

I see no objection to forming the name of a group that is at any taxonomic level between subfamily and tribe (or whatever the main category between subfamily and genus may be called) as though it were a subfamily.

Para. **36**

My only objection to the use of the termination " -INI " for the main category between subfamily and genus is that in speech it is not easy to distinguish from -INAE; and I do not think that this is a very serious objection. If, however, any change in termination were to be made in view of Professor Jeannel's claim (Paragraph 31) that " -INAE " is merely the feminine equivalent of " -INI," I consider that a change of the termination " -INI " would be less harmful than a change of the termination " -INAE " which is so much more firmly established.

Whether or no the name tribe should be employed for the main category between subfamily and genus need not affect employment of the termination "-INI," for this termination has not generally been associated with the term tribe when employed at other levels.

Whatever term is adopted for the main category between subfamily and genus should be carefully defined ; and names at this level should be co-ordinate with the names of families and subfamilies.

Para. **37**

I agree that the optional category "supergenus," when employed should be regarded as a category of the "family" group.

Para. **38**

I see no objection to prescribing that a name given to a supergenus should be treated as co-ordinate with other categories of the family group ; nor to the proviso that supergeneric names should rank for priority *inter se* by reference to the dates on which they were respectively published as such. Style for double attribution sometimes necessitated will need to be specified ; the style adopted for super-generic names should be comparable to that adopted for superfamily names—see comment on paragraphs 39-42.

I agree that it would be well for those who employ the category supergenus to put forward suggestions for a uniform termination.

Paras. **39-42**

I am not satisfied that superfamily names should be "based invariably upon the stem of the name of a genus which is already the type genus of one of the families included in the superfamily concerned." In Dibranchia, categories are recognised at one or two levels between suborder and family, and most of the names concerned are not based on the stem of the name of a genus which is already the type genus of one of the families concerned ; only perhaps one of these names has hitherto been explicitly claimed as a superfamily, but this rank seems the obvious one for names that constitute the main category between suborder and family.

Further, I am not convinced that it is desirable to include the category superfamily in the family group rather than in the ordinal group of categories, and that the names of superfamilies should be co-ordinate with the names of families.

If, however, it is indeed decided that superfamilies are to be included in the family group of categories, that their names are invariably to be based on the names of genera that are type genera of included families, and that their names are in general to be co-ordinate with the names of families, it will be more than ever necessary to provide, between suborder and superfamily, additional optional categories belonging to the ordinal group of categories.

In any event I agree that superfamily names should rank for priority *inter se* by reference to the dates on which they were severally published as such. Style for double attribution sometimes necessitated will need to be specified. " X-acea (X-idae author A, earlier date) author B, later date " would be an appropriate style if superfamilies were accepted as belonging to the family group.

Para. **40**

I agree that there should be a special termination for superfamily names but should prefer, instead of " -OIDEA," the termination " -ACEA," which has been used for superfamilies in Ammonoidea, Lamellibranchia, Gastropoda and Brachiopoda. Formerly " -ACEA " was used as a family name by Philippi, 1853, in his work on Mollusca ; now that " -ACEA " is no longer required for families, its use for superfamilies would seem very appropriate.

The termination " -OIDEA " has, I think, been used more often for higher categories (e.g. suborders or orders in Dibranchia, Brachiopoda, Carnivora ; classes in Echinoderma) than for superfamilies. For superfamilies the terminations " -OIDAE " and " -ACEA " have also been used.

Paras. **43-45**

I support the proposal that an *Official List of Family Names in Zoology*, and a complementary *Official Index of Rejected and Invalid Family Names*, should be established with scope suggested. I agree also that the consequential additions should be made to the regulations in relation to the *Official List of Generic Names*.

Para. **46**

I agree that names that belong to all the categories mentioned in this paragraph (with the possible exception of superfamilies) should be regarded as co-ordinate and eligible for inclusion in the proposed *Official List of Family Names* ; and that they should be entered in the form suggested.

Paras. **47** *and* **48**

I agree that the suggested titles of *List* and *Index* are sufficient, and that the arrangements to mark the official status of *List* and *Index* should be as suggested.

(2) Letter dated 9th June 1952

Since I wrote on May 26th I have remembered another example of a family name not based upon the name of a genus included in the family ; and as this example differs somewhat in character from the examples that I noted in commenting previously on your paragraph 22, it may perhaps be worth adding.

The family name TEUTHIDAE Owen, 1836, was based explicitly on "the term τευθος applied by Aristotle to the ten-armed Malakia with an internal horny plate or *gladius*." Even before 1836, use of the component " -teuthis " as an element in compounding generic names in Dibranchia had already begun ; and " -teuthis " or " Teuth-" now forms part of a very great number of names of genera and of higher categories in this subclass. However, *Teuthis* as a valid generic name in zoological nomenclature relates, unfortunate though the fact may appear as regards work on Dibranchia, to a quite different form that belongs not to Dibranchia but to Pisces : *Teuthis* Linnaeus, 1766, with type *T. javus* Linnaeus. This is in the *Official List of Generic Names in Zoology*—see *Opinion* 93 (1926, *Smithson. misc. Coll.* **73** [No. 4] : 5, 10), where at any rate in the published version *Teuthis* as a homonymous generic name in Dibranchia was not even mentioned. Apart from " *Teuthis* Schneider, 1784," which as you have pointed out is a cheironym possessing no status under the Rules, and which is in the *Official Index of Rejected and Invalid Generic Names*, the name *Teuthis* as a formal generic name in Dibranchia apparently does not date from earlier than J. E. Gray, 1849.

It is clear, accordingly, that the family name TEUTHIDAE Owen, 1836, was not based upon the name of a genus included in the family : for though there was in existence a valid nominal genus *Teuthis* Linnaeus, 1766, TEUTHIDAE Owen had nothing to do with that nominal genus ; and though *Teuthis* Gray, 1849, was based, like TEUTHIDAE Owen, 1836, on Aristotle's teuthis, *Teuthis* Gray, 1849, had not been proposed at the time when TEUTHIDAE Owen, 1836, was proposed. In this instance, moreover, there can be no question of retrospective validation of the family name on subsequent proposal of the appropriate generic name, as in any event *Teuthis* Gray, 1849, is preoccupied by *Teuthis* Linnaeus, 1766.

DOCUMENT 3/21

By J. R. DYMOND

(*University of Toronto, Department of Zoology, Toronto, Canada*)

Statement received on 27th May 1952

I approve the suggestions contained in Volume 7, Part 3, of the *Bulletin of Zoological Nomenclature* for the insertion in the *Règles* of provisions relating to the naming of families and cognate groups in replacement of the provisions in the existing Articles 4 and 5.

I am especially impressed by the suggestion for the creation of an *Official List of the Names of Families, Superfamilies, Subfamilies, Tribes and Supergenera.*

I also favour references to the International Commission of problems where leaving the decision to the subjective opinion of taxonomists might result in an undesirable number of changes in names.

DOCUMENT 3/22

By JAMES A. SLATER and JEAN L. LAFFOON

(*Department of Zoology and Entomology, Iowa State College of Agriculture and Mechanic Arts, Ames, Iowa, U.S.A.*)

Letter dated 28th May 1952

We believe that Articles 4 and 5 of the Rules have been misinterpreted in the past, and that a more accurate interpretation would be a great aid in the stabilisation of family names. Current practice is inconsistent with respect to treatment of the type genera of families as compared with the type species of genera. We feel that there are grounds in the Rules for removal of this inconsistency, and at the same time for moving toward a more stable basis for family names.

To illustrate our viewpoint we may first examine current practice as to type species of genera :—

(1) The type species of a genus is the first species validly designated as the type species, even though its name may be considered as a synonym of another specific name.

(2) Thus if Jones, 1910 describes *A-us*, and designates *A-us albus* Jones, 1910, as the type species, *A-us albus* Jones, 1910, remains the type species of *A-us* even though it might later be considered to be identical with, and therefore its name a synonym of *B-us cinereus* Smith, 1900.

(3) This is the current general practice with regard to type species of genera, and is supported by statements of the Commission. For example, in *Opinion* 44, *Leptocephalus morrisii* Gmelin is considered as the type species of *Leptocephalus*, even though its name is a junior synonym of *Muraena conger* L. This procedure is to be

desired, as conceivably it might at some time be found that *morrisii* and *conger* are not identical with one another and not even members of the same genus, in which case the genus name *Leptocephalus* would follow *morrisii*. On the other hand, if *conger* became the type species of *Leptocephalus* when *morrisii* became a synonym of *conger*, confusion would result if the two should later be placed in separate genera.

We may now examine the current practice relating to the type genera of families, and the portions of the Rules which are pertinent.

(1) Article 4 of the Rules states: "The name of a family is formed by adding the ending *idae*, the name of a subfamily by adding *inae*, to the stem of the name of its type genus."

(2) *Opinion* 141 states, in part: "The fact that a given generic name is selected to form the name of a family constitutes *ipso facto* a definite designation of that genus as the type genus of a family." The type genera of most families have been selected in this manner.

(3) Article 5 of the Rules states: "The name of a family or subfamily is to be changed when the name of its type genus is changed."

(4) Thus if Jones, 1850 describes a family X-IDAE, based on *X-us* Smith, 1810, the latter is automatically the type genus of family X-IDAE.

(5) According to present practice, supported by statements of the Commission,* if it were later considered that *X-us* Smith, 1810 is a synonym of *Y-us* Doe, 1800, the family name is "changed" to Y-IDAE.

(6) It is our view that the reasoning in paragraph 5 is inconsistent with Article 5 of the Rules and also with the current practice regarding type species of genera, since we would regard *X-us* as continuing to be the type genus of the family, even though it had fallen into synonymy. And, since the family name is formed by adding the termination "-IDAE" to the stem of the type genus, the family name would continue to be X-IDAE. *X-us* was the original type genus of the family, and placing it in synonymy with *Y-us* on subjective grounds does not constitute a change in the name of the type genus in the sense of Article 5. Except when *X-us* and *Y-us* have the same type species. *X-us* is purely a subjective synonym of *Y-us*, and the fact that some authors might consider it to be a synonym is no reason why a later author might not remove it from *Y-us*. In the latter case the family name would originally have been X-idae, later Y-idae, and (when *X-us* was removed from *Y-us*) again X-IDAE, according to present practice. We hold that for nomenclatorial purposes the type genus of a family should

*For example, in *Opinion* 139, p. 42, it is mentioned that since *Astatus* Jurine, 1801, had been suppressed, "It followed also that the name CEPHIDAE replaced the name ASTATIDAE." In this case *Cephus* was the first genus in the family to be made the basis of a name above genus—and the obvious implication of the Commission was that if *Cephus* had been allowed to fall as a synonym of *Astatus*, the family name would have to be changed to ASTATIDAE, since the name of the type genus would have been "changed."

always remain the original nominal genus so designated, and not follow the vagaries of name "changes" due to shifting ideas as to synonymy. (An especially horrible example of the latter is the case in Diptera of the changing practice in citing a family name for the family once known as RHYPHIDAE, later ANISOPODIDAE, then PHRYNEIDAE and then SYLVICOLIDAE—when, in our view, the name should have remained RHYPHIDAE all the time, since *Rhypus* is the type genus.)

(7) There remains to be explained what cases would be covered by Article 5; i.e., if it is assumed that the name of the type genus does not change for nomenclatorial purposes just because it falls into synonymy—when is it changed in the sense of Article 5? We believe that this would apply only to cases where the original type genus (the *X-us* Smith, 1810, of our example) is found to be a junior homonym, or other grounds which would make it unavailable (such as being a nude name, or suppressive action by the Commission). The above interpretation of the type genus would be in accord with current practice in respect to type species.

(8) We agree that this interpretation of Articles 4 and 5 is possibly not in accord with the intent of the writers of the Rules (judging from some of the *Opinions* which indirectly treat on this matter), yet we believe that a strict interpretation of the wording of these Articles can lead only to the interpretations we have made. It is interesting to note that several authors have advocated changes in the Rules in order to allow the use of generic names in synonymy as the basis for formation of family names in at least some cases (e.g. Sabrosky, 1947, *Amer. Nat.* **81**: 153-160; and Hemming, 1952, *Bull. zool. Nomencl.* **7**: 74-75) but to our knowledge no previous writers have noted that the basis for such action is already present in the Rules. We have had correspondence with several specialists who have indicated that they would like to be able to use generic names in synonymy as the stems for family names.

We are of the opinion that this interpretation of Articles 4 and 5 would be a great step forward in the stabilisation of family names. No doubt some family names that have been changed in the past would have to be switched back to an earlier usage (unless the Commission used its plenary powers where the present family name has come into wide acceptance), but many future changes could be avoided.

One case in point is the family names in Diptera revolving around the acceptance as valid published names of Meigen's 1800 genera. We believe that, if what we consider to be the proper interpretation of type genera had been made by past workers, that, for example, the family name MYCETOPHILIDAE would be in current universal use even though *Mycetophila* must be considered a synonym of *Fungivora* (in this case, both genera have the same type species). It is highly probable that, if dipterists had realised that it was not necessary to change the several family names involved even though the Meigen, 1800 generic names were accepted, that less opposition would have developed against the generic names.

DOCUMENT 3/23

By S. G. KIRIAKOFF, L.SC.
(*Zoological Museum, Ghent University, Ghent, Belgium*)

Enclosure to letter dated 5th June 1952

Commission's Ref. Z.N.(S.)357. Formation of the Names of Families and Subordinal Categories of Suprageneric Rank (" Bull. zool. Nomencl.," 7 (pt. 3) : 61-94)

I agree with the suggestions contained in the above paper.

I submit the following suggestions to be considered by the Commission :—

(a) Terminations of superfamilies to be " -OIDEA " ;

(b) Terminations of subfamilies to remain " -INAE " ;

(Note.—Professor Jeannel's objection* to the feminine form of " -INAE " does not stand, because the family termination " -IDAE " is also feminine and to the latter he does not object ; moreover, the masculine termination " -INI " for tribes has so far no legal status.)

(c) Terminations of supertribes to be " -INES " ;

(d) Terminations of tribes to be " -INI " ;

(e) Terminations of subtribes to be " -IDI."

For my suggestion regarding the cohortal category, see separate sheet enclosed herewith.†

Supplementary Note

Editorial Note : Attention is drawn to a paper dealing jointly with the problem of Family names and Ordinal names which forms an annexe to a communication on the latter subject received from Dr. S. G. Kiriakoff (*Zoological Museum, Ghent University, Belgium*), which appears in the Ordinal names section of present Copenhagen series of papers.‡ In this paper Dr. Kiriakoff expresses the view that the proposed revision of the rules relating to the naming of families and allied groups, which he supports, should be supplemented by the inclusion therein of provisions relating to the naming of the sub-category " cohort " and the divisions just above and below it, namely the " supercohort " and the " subcohort," there being, in his experience, cases where " the use of Superfamilies alone would have been quite inadequate to express the nearer relation between the various phyletic stems and branches, each consisting of one or more Superfamilies."

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*See Document 3/9.
†See the Supplementary Note annexed.
‡See Document 4/13.

DOCUMENT 3/24

By ERICH M. HERING

(*Abteilungsleiter am Zoologischen Museum der Humboldt-Universität zu Berlin*)

Statement received on 7th June 1952

Stellungnahme zu den Vorschlägen zur Erklärung und Erweiterung der "Règles" in Bezug auf die Bennungung der Familien, etc.

Der Zoologen-Stab des Zoologischen Museum Berlin unterstützt mit 14 von 14 Stimmen die im Bull. Zool. Nom. vol. 7, Pt. 3, p. 61-94 ausgeführten Vorschläge mit den folgenden besonderen Bemerkungen :

(1) Para. 11 (l. c. p. 70) : Typisches Genus der Familie soll das vom ersten Auswählenden festgelegte Genus sein, das nicht das älteste Genus der Familie zu sein braucht.

(2) Para. 12 (l. c. p. 71-73) : Unterstützt wird der Vorschlag, dass für das Prioritätsdatum bis etwa 1850 auch Namensbildungen ohne die Endung -idae, von diesem Zeitpunkt an nur solche mit der Endung -idae Priorität haben sollen.

(3) Para. 16 (p. 75) : Bei Vereinigung von 2 und mehr Familien soll der este auswählende Autor das Recht haben, den Namen der neuen Familie festzulegen.

(4) Para. 22 (p. 79-80) : Für Zulassung von Ausnahmen von der Regel, dass der Name der Familie vom Stamm der typischen Gattung gebildet wird, sind nur 11 von 14 Stimmen abgegeben worden.

Annexe to Statement furnished by Professor Dr. Hering

Views of Dr. WALTER FORSTER

(*Zoologische Sammlung des Bayerischen Staates. München. Germany*)

Herr Dr. Walter Forster von der Zoologischen Sammlung des Bayerischen Staates teilt als Ergebnis der Befragung von 12 Mitgliedern des zoologischen Stabes des Museums München mit, dass die genannten Vorschläge mit 11 von 12 Stimmen unterstützt werden, mit den folgenden Bemerkungen :

(1) Para. 11 : Das Recht der Festlegung des typischen Genus für die Familie hat der erste auswählende Autor.

(2) Para. 16 : Bei Vereinigung von Familien soll der neue Familienname der Name der Familie mit dem ältesten Genus sein.

(3) Para. 22 : Die Zulassung von Ausnahmen wird unterstützt.

DOCUMENT 3/25

By RAYMOND C. MOORE
(*University of Kansas, Lawrence, Kansas, U.S.A.*)

Letter dated 22nd June 1953

You should receive by any day—perhaps already have—copy of a Circular which I asked my secretary in Lawrence to mail you when she distributed copies to TREATISE authors. This deals with the subject of nomenclature of familial categories primarily and represents my considered judgments set forth as basis for handling organisation of TREATISE taxonomic manuscripts. You will see that reference is duly made to proposals published by you in the *Bulletin of Zoological Nomenclature*, a majority of your recommendations intended for consideration of zoologists and attention by the Copenhagen Congress being adopted. I dissent from the form of citation which you have proposed for emended familial names in some particulars, chiefly because it seems that your form constitutes in effect an attribution retrospectively to original authors. This is objectionable, although the principle of ascribing equivalence to date of original publication (based on choice of an indicated type genus) for purposes of priority, seems very desirable and sound.

I should like especially to call to your attention the matter of recommended terminations for various familial categories, as given in the Circular mentioned :

Superfamily, -icae	Supertribe, -ices
Family, -idae	Tribe, -ides
Subfamily, -inae	Subtribe, -ines

This scheme, independently devised by some American zoologists, I learn, has advantages of consistency and mnemonic nature. In paleontology, the superfamily ending -acea (not noticed in your paper) has so far had widest general adoption. I find that various TREATISE authors already approached have readily agreed to use -icae for superfamily assemblages, and I think that this will be adopted generally, if not exclusively, in the TREATISE. I have not undertaken to impose " law " in this regard, when none yet exists.

Annexe to Professor Raymond C. Moore's Letter of 22nd June 1952

TREATISE ON INVERTEBRATE PALEONTOLOGY

Directed and edited by Raymond C. Moore

CIRCULAR 6, SECTIONS 1-4, 10th JUNE 1952

Outline and Summary

1. INTRODUCTION. Purpose of circular is mainly to outline *Treatise* procedure in dealing with familial names. Brief notes are added concerning preparation of illustrations, abbreviation of authors' names, and progress of the project (p. 4).

2. HEMMING PROPOSALS ON NOMENCLATURAL PROCEDURE FOR SUPRAGENERIC CATEGORIES. Revision of Rules suggested for action by Copenhagen (1953) Congress; provides for recognition of six categories between genus and suborder, all treated as nomenclatorially co-ordinate, and suggests codification of nomenclature applied to divisions higher than superfamily in rank (p. 4).

3. NOMENCLATURAL PROCEDURE FOR FAMILIAL CATEGORIES IN TREATISE.

3.1. *Categories of familial names.* Dependent on taxonomic requirements, authors may recognise at their option assemblages of genera classed as: (*a*) superfamily, (*b*) family, (*c*) subfamily, (*d*) supertribe, (*e*) tribe, and (*f*) subtribe (p. 5).

3.2. *Formation of familial names.* To the stem of the name of a chosen type genus endings which specify individual familial categories are to be added (p. 5).

3.21. *Recognition of generic stem.* In names of classic origin, the stem normally consists of the genitive form less the terminal vowel (or diphthong) or terminal vowel followed by consonant. The stem of indeclinable names comprises the whole name (p. 5).

3.22. *Endings of familial names.* Besides prescribed endings (marked *), those recommended for adoption in the *Treatise* are :—

Superfamily, -icae	Supertribe, -ices	
*Family, -idae	Tribe, -ides	
*Subfamily, -inae	Subtribe, -ines	(p. 6)

3.23. *Names not available for familial assemblages.* These include: (*a*) names not Latin or Latinized, (*b*) names not founded on generic names, (*c*) names not originally of suprageneric rank, (*d*) junior homonyms and synonyms, (*e*) names based on a type other than that having priority of designation, and (*f*) names based on invalid emendations of generic names (p. 7).

3.3. *Inter-relationships of familial names.* The various categories of familial names are to be treated as nomenclatorially co-ordinate (p. 8).

3.31. *Superfamily names.* Except that authorship and priority are based on original publication as proposed superfamily, this category is subject to same rules as apply to other familial assemblages (p. 8).

3.32. *Family and subfamily names.* A name originally published with ending -idae may be changed to -inae, or vice versa, author and date being cited as of the original publication (p. 8).

3.33. *Supertribe, tribe, and subtribe names.* Names published for any of these categories, consisting of the stem of the name of the chosen type genus combined with a letter or letters to form a selected ending, may be employed to designate a subfamily by substitution of -inae for the ending of the name published, or to designate a family by substitution of -idae for the ending of the name published. Likewise, changed assignment of rank among supertribe, tribe, and subtride may be made at will, with change of endings as desired. Author and date cited for the assemblage revised in rank or in form of ending without change in rank are as in the original publication (p. 9).

3.4. *Priority as guide in selection of familial names.* The first-published name shall be accepted unless it is unavailable. An effect of recognising co-ordinate status of familial names is that the stem of the generic name first published in designating a subfamily or division of a subfamily must be used also in designating the family to which it belongs (p. 9).

3.5. *Emendation of familial names.* Change from originally published form of familial names is required (*a*) if the stem of the generic name is incorrectly used, (*b*) if the name of the type genus has been altered, and (*c*) if taxonomic rank assigned to the assemblage is changed as regards family and subfamily categories ; in addition, (*d*) change in form of endings of superfamily, supertribe, tribe, and subtribe categories is allowable and may be judged desirable (p. 9).

3.51. *Correction of stem.* This involves no change of stem but is necessary to co-ordinate familial names with those of type genera (Belosepiidae Gill, 1871, based on *Belosaepia*, must be emended to Belosaepiidae) (p. 9).

3.52. *Change of stem.* If the name of the type genus of a familial assemblage must be changed, as by reason of homonymy, the familial name also must be altered correspondingly (Geoteuthidae, Naef, 1921, based on *Geoteuthis* Naef, 1921 [*non* Münster, 1843], altered to Geopeltidae Altena, 1949, based on *Geopeltis* Altena, 1949, which replaced *Geoteuthis* Naef) (p. 9).

3.53. *Change of ending.* When taxonomic reassignment of a familial assemblage involves the categories of family or subfamily, change of ending of the familial name is mandatory. For categories other than family and subfamily, emendation in the form of endings of familial names is optional (p. 10).

3.6. *Citation of author and date of familial names.* The Rules stipulate that when the author of a zoological name is cited, no punctuation of any sort should intervene between zoological name and author's name, but the date may be enclosed in parentheses or separated by comma. Citation of some familial names offers problems, as follows (p. 10).

3.61. *Names emended by correction of stem.* Retrospective attribution of such changes to the original author and date, although considered allowable by some, is judged unacceptable, but because no change of type genus is introduced, the emended familial name is assigned priority status of the incorrect original name. Example of form of *Treatise* citation: Belosaepiidae Bairstow, 1952 (=emend. Belosepiidae Gill, 1871) (p. 11).

3.62. *Names emended by change of stem.* Retrospective attribution of such changes to the original author and date is agreed by almost everyone to be unacceptable, but because no change in identity of the type genus is introduced, the emended familial name is assigned priority status of the nomenclatorially invalid original name. Example of form of *Treatise* citation: Geopeltidae Altena, 1949 (=emend. Geoteuthidae Naef, 1921) (p. 11).

3.63. *Names emended only by change of ending.* Citation should be to original author and date. Recommended *Treatise* form: Aulacoceratidae Mojsisovics, 1902 (as Aulacoceratinae) (p. 11).

3.64. *Superfamily names.* Author and date to be cited should refer to earliest proposal of the superfamily name as such, but if ending is altered, *Treatise* form recommended is: Fungiicae Vaughan & Wells, 1943 (as Fungioidea) (p. 12).

3.65. *Family names.* Author and date to be cited should refer to first-published proposal of family or suprageneric subdivision of family, modified in manner given by 3.61, 3.62, or 3.63 if called for (p. 12).

3.66. *Subfamily names.* Author and date to be cited should refer to oldest publication in which any genus assigned to the subfamily was proposed as type of a familial assemblage. A nominotypical subfamily has the same type genus as that belonging to the family, and citation of such subfamily refers to the same author and date as for the family; whether the author of the family recognised subfamilies or not is immaterial. Example: the family Astrocoeniidae Koby, 1890, contains the nominotypical subfamily Astrocoeniinae Koby, 1890 (not to be cited as Astrocoeniinae Felix, 1898, although he first published such subfamily name) (p. 12).

3.67. *Supertribal, tribal, and subtribal names.* Considerations stated in 3.66 apply in identical manner to each category of these names (p. 13).

3.7. *Examples of procedure in nomenclature of familial assemblages* (p. 13).

4. NOMENCLATURAL PROCEDURE FOR SUPRAFAMILIAL CATEGORIES IN TREATISE. Authors are to use own judgment; no standard endings or other features of nomenclature (p. 14).

. . . . . .

1. Introduction

Preparation and distribution of this Circular has been delayed unavoidably by pressures on the Editor while absent from the University of Kansas. Although nearly full-time work by his secretary, Mrs. Ruth Breazeal, has been devoted to *Treatise* matters, difficulties are evident when one remembers that she is in Lawrence and since July 1951 the Editor has been in Europe, with only a portable typewriter and a supply of paper to aid him. The volume of work done is measured only vaguely by statement that postage costs at the Editor's end of the line have averaged nearly 40 dollars per month, mostly but not all on *Treatise* business. As much time as possible has been given to various manuscripts. Questions brought to attention by some of this work, as well as problems broached in correspondence, are treated in the present Circular. Before decisions could be reached, however, many letters had to be written for the purpose of consultations. Special effort has been made to avoid hasty conclusions which might lead to unwise policies in shaping taxonomic sections of *Treatise* manuscripts. Authors are asked to co-operate with the Editor in achieving uniformity among the various divisions of the *Treatise* by adopting procedures set forth in Circulars 5 and 6. Presumably, it is unnecessary to add that this is a request, rather than a direction, for if an author dissents, he may proceed otherwise, but in such case concise written statement of reasons for the dissent should be submitted.

2. Hemming Proposals on Nomenclatural Procedure for Suprageneric Categories

Many questions arise, especially in dealing with suprageneric taxonomic categories, concerning which the Rules are virtually silent. The March 1952 issue of the *Bulletin of Zoological Nomenclature* (vol. 7, pt. 3, pp. 61-94) contains proposed clarification, amendment, and expansion of the Rules relating to names of families and subordinate categories of suprageneric rank, but these proposals are simply suggestions offered as basis for advice sought from zoologists on formulating provisions which may be adopted by the International Congress of Zoology scheduled to meet in Copenhagen in 1953. The *Treatise* cannot wait on adoptions made at this Congress, even though parts of the *Treatise* will not yet be finished at that time. We must formulate our own procedures as well as we can, at least for first-issued parts of our publication, trusting that they are not greatly different from Rules which ultimately will be introduced. The draft of proposals by Mr. Francis Hemming, Secretary of the International Commission on Zoological Nomenclature, includes superfamilies in the bracket of categories treated in the reference just given, and he recommends that name of superfamilies should be co-ordinate with names published as family, subfamily, and smaller suprageneric groups (p. 89).

Through kindness of Mr. Hemming, galley proof of a paper drafted by him for publication in *B.Z.N.* later in 1952 has been furnished to the *Treatise* Editor. This deals with problems requiring consideration if naming of taxonomic categories higher in rank than superfamilies is to be included among provisions of the Rules. The contents of this discussion are of smaller concern to *Treatise* authors than that dealing with suprageneric categories ranging upward to

superfamilies, because resolution of problems and formulation of acceptable Rules for naming phyla, subphyla, classes, subclasses, orders, and suborders are not likely to be achieved soon. Many paleontologists and other zoologists judge that nomenclature of ordinal and higher categories should not be subjected to stipulation imposed by Rules.

3. Nomenclatural Procedure for Familial Categories in Treatise

3.1. *Categories of Familial Names*

Families and *subfamilies* are recognised in the current Rules. Assemblages of families designated as *superfamilies* have come to be recognised widely in many divisions of the animal kingdom and provisions concerning their nomenclature undoubtedly will be incorporated ultimately in revised Rules. Also, taxonomists working on many groups have found need to recognise groups of genera within subfamilies, such groups being presumed to be somewhat closely inter-related and having phylogenetic significance ; commonly, the groups judged to be intermediate in rank between genus and subfamily are termed *tribes*. A few divisions of animals, chiefly arthropods and mollusks among invertebrates, may call for still other categories below rank of subfamily and above that of genus ; these are appropriately termed *supertribe* and *subtribe*. The term supergenus, used by some zoologists, is considered to be synonymous with subtribe, the latter being preferable. Although present Rules do not recognise these tribal categories, it is very likely that they will be incorporated in provisions of future Rules (*B.Z.N.*, vol. 7, pt. 3, pp. 86-89). Thus, listing of familial categories in descending order of rank may be given as (*a*) superfamily, (*b*) family, (*c*) subfamily, (*d*) supertribe, (*e*) tribe, (*f*) subtribe. The expression " familial names " will be used in this Circular to designate all categories of names from subtribe (or supergenus) to superfamily, inclusive.

3.2. *Formation of Familial Names*

The Rules now provide for the manner of forming family and subfamily names but not other familial names. The following suggestions accord with regulations judged likely to be incorporated in revised Rules (except for unforeseeable stipulations as to terminations of various categories which may be adopted) and to the extent that they are accepted by *Treatise* authors, uniformity of procedure will appear in our publication. A general requirement is that all categories of familial names shall be formed by addition of some letter combination as ending, this combination being joined to the stem of the name of a generic name which is (directly or indirectly) type of the assemblage.

3.21. *Recognition of generic stem.* An essential feature in construction of familial names is correct determination of the stem of the generic name, because addition of -idae, -inae, or other adopted endings for familial categories to differently conceived stems leads obviously to different familial names. For example, published family names Streptelasmidae, Streptelasmaidae, Streptelasmatidae all employ the ending -idae joined to differently conceived stems of the type genus *Streptelasma*. The problem of correct determination of the

generic stem is mostly a simple one, but unfortunately difficulties and errors are numerous. In generic names consisting of classic words (Greek or Latin), compounds of such words, or modeled in form on such words (Latinized), the rule for determination of the stem is to take what is left after dropping the terminal syllable (vowel or diphthong, or terminal vowel followed by consonant or consonants) from the genitive form of the word. Various classical words, however, are indeclinable and the stem comprises all of such words; generic names consisting of barbaric words or arbitrary combinations of letters likewise generally are indeclinable. The following tabulation gives some examples in which the terminal syllable of the name and its genitive form is underlined* and the stem of a family name based on it is printed in capitals; words of most common type are marked by an asterisk.

Name and gender	*Genitive form*	*Family name*
Abb*a* (m)	Abbat*is*	ABBATidae
*Abamit*a* (f)	Abamit*ae*	ABAMITidae
Ablegmin*a* (n)	Ablegmin*um*	ABLEGMINidae
Acroam*a* (n)	Acroamit*is*	ACROAMITidae
Abazea (n)	Abazeor*um*	ABAZEORidae
Acarnan (m)	Acarnan*is*	ACARNANidae
Ab*as* (m)	Abant*is*	ABANTidae
Abb*as* (m)	Abbat*is*	ABBATidae
*Abanti*as* (f)	Abantiad*is*	ABANTIADidae
*Absurdit*as* (f)	Absurditat*is*	ABSURDITATidae
Abraxas (f)	Abraxae	ABRAXidae
*Ab*ax* (m)	Abac*is*	ABACidae
Abal*e* (f)	Abal*es*	ABALidae
*Abdomen (n)	Abdomin*is*	ABDOMINidae
*Ac*er* (n)	Acer*is*	ACERidae
Abandiad*es* (m)	Abandiad*ae*	ABANTIADidae
*Achaemenid*es* (m)	Achaemenid*is*	ACHAEMENIDidae
Absyrtid*es* (f)	Absyrtid*um*	ABSYRTIDidae
Aci*es* (f)	Aci*ei*	ACIEidae
*Abi*es* (n)	Abiet*is*	ABIETidae
Abal*i* (m)	Abalor*um*	ABALORidae
Abar*is* (m)	Abar*is*	ABARidae
Abnept*is* (f)	Abnept*is*	ABNEPTidae
*Abaesamis (f)	Abaesamid*is*	ABAESAMIDidae
*Abdicatr*ix* (f)	Abdicatric*is*	ABDICATRICidae
*Abactio (f)	Abaction*is*	ABACTIONidae
*Absumedo (f)	Absumedin*is*	ABSUMEDINidae
*Acheron (m)	Acheront*is*	ACHERONTidae
*Abactor (m)	Abactor*is*	ABACTORidae
Abnepos (m)	Abnepot*is*	ABNEPOTidae
Acanos (m)	Acan*i*	ACANidae
Acinos (f)	Acin*i*	ACINidae
*Abluvi*um* (n)	Abluv*ii*	ABLUVIidae
Acher*uns* (m)	Acherunt*is*	ACHERUNTidae

*Ed. Note.—In printing this document italics have been substituted for underlining.—F.H.

*Abac*us* (m)	Abac*i*	ABACidae
Abnur*us* (f)	Abnur*us*	ABNURidae
Acl*ys* (f)	Aclid*is*	ACLIDidae
*Elasm*a* (n)	Elasmat*os*	ELASMATidae
Abad*an* (m)	(Indeclinable)	ABADANidae
Abac*uc* (m)	,,	ABACUCidae
Abdenag*o* (m)	,,	ABDENAGOidae
Abarim*on* (m)	,,	ABARIMONidae
Abessal*om* (m)	,,	ABESSALOMidae
Abn*er* (m)	,,	ABNERidae
Abraxar*es* (m)	,,	ABRAXARESidae

3.22. *Endings of familial names.* Because the prescribed endings of names for families and subfamilies are in form of feminine plurals (-idae, -inae), the ending of names of superfamilies suggested for use in the *Treatise* has similar form. For this category, -icae is chosen because it closely follows the form of adopted family and subfamily endings, but does not resemble either of them when pronounced (for example, -itae may not be distinguishable from -idae when spoken). The endings -acea and -oidea, which have been used considerably for superfamilies do not have terminal -ae, which seems desirable, and the ending -aceae (suggested in Circ. 2 as modification of -acea) has been little used as yet in zoological nomenclature, although well known as applied to plants. The superfamily ending -oidae, used by some taxonomists, is objectionable because it conduces confusion, especially when joined to the stem of some generic names ending in -o (as ABEDNAGOidae, par. 3.21). For names of tribal categories (supertribe, tribe, subtribe) no endings have come into common use, although *B.Z.N.* (vol. 7, pt. 3, p. 87) refers to habitual use of -ini as termination of the names of tribes. This seems objectionable because it is indistinguishable from -inae when pronounced; also, memory is aided by employing comparable letter combinations to denote "super," "main," and "sub" categories. The schedule here suggested is shown in the following tabulation, but attention is called to the point that adoption by *Treatise* authors is voluntary.

Superfamily, -icae	Supertribe, -ices
Family, -idae	Tribe, -ides
Subfamily, -inae	Subtribe, -ines

Examples: Superfamily Stauriicae; Family Stauriidae; Subfamily Stauriinae (genus *Stauria*).

3.23. *Names not available for familial assemblages.* Although present Rules do not offer clear guidance on questions of admissibility of various names published for familial categories, the following statements of policy are proposed for adoption by *Treatise* authors.

3.231. *Names not Latin or Latinized.* According to Art. 3, the scientific names of animals must be words that are Latin or Latinized. Thus, designations like orthids (English), Pisokrinoiden (German), and Aulacocératidés (French) are not admissible as familial names; likewise Famille Syringoporiens Fromentel, 1861, cannot be accepted without emendation.

When and if such non-Latin names are published in Latinized form, they should not be attributed retrospectively to the author who used the non-Latin name, but to the subsequent person who first published an acceptable Latinized word.

3.232. *Names not founded on generic names.* The stipulation given in Art. 4 of the Rules that names of families and subfamilies must be formed by adding specified endings to the stem of a generic name serves to invalidate familial names formed otherwise, and provision that names of all familial categories shall be treated as co-ordinate (see paragraph 3) extends the stipulations to superfamily and tribal names. Thus, Hastatidae Stolley, 1919, is not valid because there is no corresponding generic name (derivation presumably based on a " section " of broad genus *Belemnites* called Hastati, after the species *Hibolites hastatus*).

3.233. *Names not originally of suprageneric rank.* Art. 4 implies that names of generic or lower rank are ineligible as names of familial categories (although the stem of a generic name is used in forming such familial names). Thus, subdivisions of a genus called Clavati, Canaliculati, Hastati, etc., based on the trivial names of species, cannot be accepted as suprageneric names.

3.234. *Junior homonyms and synonyms.* Familial names formed from the stem of generic or subgeneric names which are themselves not accepted as valid in zoological nomenclature are naturally inadmissible. Such invalid generic and subgeneric names include junior homonyms and junior objective synonyms. Not necessarily invalid are junior subjective synonyms, but generic and subgeneric names in this category are not acceptable as bases for familial names unless they are removed from synonymy.

3.235. *Names based on a type other than that having priority of designation.* This does not mean necessary choice of the first-published generic or subgeneric name among those included in a familial assemblage, but that having priority of designation as type of such an assemblage.

3.236. *Names based on invalid emendations of generic and subgeneric names.* So exceptional are circumstances which allow change of any sort in form of a generic or subgeneric name as originally published that a safe, general rule is to consider emendation invariably unallowable. This means that the originally published name, whatever its faults, is the only valid name (see Circ. 4, p. 9). A recent discussion by R. Richter (*Senckenbergiana*, vol. 32, pp. 357-366, 1952), which contains numerous examples of adjudged invalid emendations, develops the theme " Quo modo impressum est nomen maneat "—in whatever manner a name is printed, let it stand. This extends in Richter's view, even to incorrectly spelled dedication of names to specified persons (for example, *Zittelloceras* Hyatt should retain this spelling, even though original description indicates that the genus is named for Karl von Zittel). Unless emendations are made by action of the Commission on Zoological Nomenclature, stability of nomenclature is disturbed. Attention may be called to the point that names derived from languages using diacritical marks involve several special problems, some of which

are complicated by unavailability of type characters for diacritically marked letters in many printing establishments. Whenever possible, diacritical marks should be employed as enjoined by Art. 20 of the Rules, but it is altogether permissible to substitute *oe* for ö or ø, *ue* for ü, and *aa* for å. As emphasised by Richter, such letters as ö, ø, ü, and å are wholly different characters from *o*, *u*, and *a* and cannot be transliterated or replaced by these letters. (For example, *Müllería* may be written as *Muelleria*, but not as *Mulleria* ; *Westergårdia* may be written as *Westergaardia*, but not as *Westergardia*.) The import of this subparagraph is that an invalid emendation of *Zittelloceras* to *Zitteloceras* cannot be recognised in forming a familial name (Zittelloceratidae being correct and Zitteloceratidae being incorrect).

3.3. *Inter-relationships of Familial Names*

It is desirable to give attention next to inter-relationships for nomenclatural purposes of familial names recognised.

3.31. *Superfamily names.* Like other familial names, a proper requirement is that the name of a superfamily shall be formed by adding a termination other than -inae and -idae to the stem of the generic name which constitutes the type of the type family of the superfamily. It seems logical to propose (as in *B.Z.N.*, vol. 7, pt. 3, pp. 89, 90) that the names of superfamilies should be treated as co-ordinate with family names and that except for qualification of holding rank for priority by reference to their dates of publication as such (not by reference to the dates of publication of their type families), rules applied to names of superfamilies should be the same as for names of families. Procedure in this manner is suggested as *Treatise* policy.

3.32. *Family and subfamily names.* Although Art. 6 of the Rules prescribes that generic and subgeneric names are co-ordinate with one another and Art. 11 makes similar provision in respect to specific and subspecific trivial names, no such stipulation is given for family and subfamily names. Logical extension of existent Rules, however, strongly supports general practice in regarding family and subfamily names as co-ordinate ; revised Rules almost surely will establish formally such co-ordination. Thus, a name originally published as a family name, ending in -idae, may subsequently be recognised as a subfamily name by changing the termination to -inae, and a name originally published as a subfamily name, ending in -inae, may subsequently be recognised as a family name by changing the termination to -idae ; author and date of establishing these units remain unchanged (see *B.Z.N.*, vol. 7, pt. 3, pp. 85-86).

3.33. *Supertribe, tribe, and subtribe names.* Subject to the requirement that names published for categories belonging between subfamily and genus in rank (supertribe, tribe, and subtribe, or equivalent units) shall be formed by addition of a selected termination to the stem of the name of a chosen type genus (see paragraph 3.22), the names of these categories should be treated as co-ordinate with those of families and subfamilies (*B.Z.N.*, vol. 7, pt. 3, pp. 87, 88). Thus, if the name Mesopleurides or some other based on the stem of the generic name *Mesopleura* were published by Jones, 1890, for an assemblage of genera regarded by him as lower in rank than subfamily, a

later author might recognise this name as applicable to a subfamily called Mesopleurinae or to a family called Mesopleuridae, with citation of author and date as Jones, 1890.

3.4. *Priority as Guide in Selection of Familial Names*

The principle of priority may be accepted as most sound basis (other things being equal) for stability in nomenclature at familial levels as at lower taxonomic levels, provided by Arts. 25-30 of the present Rules. Priority of publication as criterion for selection of familial names naturally must be restricted to available names, that is, those which are judged on other grounds to be eligible. As regards determination of priority of publication among names applicable to any familial category, it is important to note that (excepting names of superfamilies) a name originally published for any one of these categories is available for others and may be recognised on grounds of priority. Changed assignment of taxonomic rank simply requires alteration of the ending attached to the stem of the name of the type genus, author and date remaining unchanged. The import of this is that (just as no generic name may embrace subgeneric names having publication dates earlier than its own) no family name can be recognised with included subfamily or tribal names having publication dates earlier than itself.

3.5. *Emendation of Familial Names*

Adoption of the principles of priority and co-ordination as applied to familial names (except for separation of superfamily names as an independent category) requires that some of these names must be emended from the form of their original publication in order to conform to a changed taxonomic rank of the familial name or to agree with change in writing the name of the type genus. Thus, two sorts of alteration of familial names must be considered : those involving the stem and those which change the ending.

3.51. *Correction of stem.* Some published familial names are invalid because they are based on invalid emendation of the name of the type genus (for example, Belosepiidae Gill, 1871, based on invalid change from *Belosaepia* Volz, 1830, to *Belosepia*). Such familial names must be corrected to agree with the stem of the name of the type genus.

3.52. *Change of stem.* Whenever the name of the type genus of a familial assemblage is emended, as by reason of synonymy or homonymy, the name of the familial group must be changed if the stem of the generic name is altered. Art. 5 of the Rules stipulates this for families and subfamilies, but the provision may be extended to lower-rank assemblages. Indirectly, names of superfamilies are included also, because they are formed on the basis of the stem of the generic name which serves as type of the type family. All changes of familial names involving change of stem constitute introduction of new names and, as in publication of new names in similar circumstances for genera and species, these new familial names should be attributed to the author who first publishes the changed name and bear date of such publication. This procedure, however, obviously is opposed to stability in nomenclature because

priority held by the rejected name is sacrificed, despite the fact that the type genus on which it and the emended familial name are based is unchanged. A familial name which has been little used and is virtually unknown, based on a different type genus, is likely to have priority over the emended name. For example, Poteriocrinidae Roemer, 1843, has been emended to Poteriocrinitidae Bassler, 1938, on the basis of generic names which are objective synonyms (founded on the same type species), but Mollocrinidae Wanner, 1916, based on another genus included in the assemblage, is older than Bassler's emended name of the family, founded on the genus chosen by Roemer. Accordingly, proposal has been made (*B.Z.N.*, vol. 7, pt. 3, pp. 73, 74, 81, 82) that for the purpose of fixing priority, an emended familial name should date from the publication in which the type genus was originally indicated and not from the date of published emendation. This seems to be a very sensible and desirable provision—that priority in publishing the type genus of a familial assemblage and fixation of such type should take precedence over ritualistic application of a rule which commonly would require change in definition of the type genus. Present rules make no provision for such procedure, but proposal is made nevertheless to adopt it in the *Treatise*. Mode of citation is treated in paragraphs 3.61 and 3.62.

3.53. *Change of ending.* Whenever the assigned taxonomic rank of a familial name is different from that indicated in the original publication, the combination of terminal letters which is employed as sign of taxonomic rank is to be changed. Also, if the taxonomic rank remains unchanged but the originally published ending of the name differs from that now recognised as applicable to such rank, the name is to be altered by substituting the adopted different ending. Neither of these changes (except under certain conditions in forming a superfamily name) is construed to affect authorship and date of the familial name ; accordingly, the name in changed form is cited without any indication that it differs from the original publication. Thus, a tribe Martiini Smith, 1922 (type, *Martia* Adams, 1901) may become the tribe Martiides Smith, 1922 (same taxonomic rank but change in adopted ending), or Martiinae Smith, 1922 (reclassifying an assemblage of genera containing *Martia* as a subfamily), or Martiidae Smith, 1922 (reclassifying an assemblage of genera containing *Martia* as a family). Of course, modifications of the content of the assemblage by additions or removals of genera, or both, do not affect nomenclature of the group which includes the chosen type genus *Martia* as name-giver. Because it is judged advisable to treat superfamilies separately for purposes of determining authorship and priority, and because the type of a superfamily is one of its contained families, the name of a superfamily is not made by *changing* the ending -idae of its type family, for this latter persists ; it is formed by *introducing* an ending chosen to indicate superfamily rank. Priority and authorship of a superfamily are to be defined by first publication as such, without reference to the letter combination used as ending of the superfamily name in the original publication. A subsequent author may change this ending, but in such instance the assemblage retains its original author and date. Thus, the superfamily Fungioidea Vaughan & Wells, 1943, (based on Fungiidae Dana, 1848) may be altered to Fungiicae Vaughan & Wells, 1943 (as is actually done by Wells in his *Treatise* manuscript).

3.6. *Citation of Author and Date of Familial Names*

For scientific names of any category, the Rules provide for citation of the author who first published the name, omitting punctuation between the familial name and name of the author ; the date of original publication may be added enclosed in parentheses or separated from the author's name by a comma (Art. 22). This seems to be simple and clear. Citation of author and date in accordance with Art. 22 should be given for first-published name. Questions arise, however, in dealing with some familial categories, especially in cases of emended names.

3.61. *Names emended by correction of stem.* Although some authors judge that familial names which are incorrectly constructed (*a*) by reason by error in recognising the stem of the name of the type genus or (*b*) because they are based on an invalid " minor " emendation of the name of the type genus, may be corrected and cited under the author and date of the original publication, such practice is deemed unacceptable in the *Treatise*. This retrospective attribution is misleading and may involve undesirable subjective elements. Recommended form of citation in the *Treatise* for emendations of (*a*) type : Loliginidae Gill, 1871 (=emend. Loligoidea Lesueur, 1821) ; Octopodidae d'Orbigny, 1838 (=emend. Octopoda Férussac, 1821). For emendations of (*b*) type : Belosaepiidae Bairstow, 1952 (=emend. Belosepiidae Gill, 1871) ; Cirroteuthidae Bairstow, 1952 (=emend. Cirrhoteuthidae Keferstein, 1865). Originally published names *Belosaepia* and *Cirroteuthis* were subsequently incorrectly changed to *Belosepia* and *Cirrhoteuthis*. For purpose of priority assignment, dates of these families are reckoned as : Loliginidae 1821, Octopodidae 1821, Belosaepiidae 1871, Cirroteuthidae 1865.

3.62. *Names emended by change of stem.* As stated in paragraph 3.52, recognition (and hence citation) of the author and date of familial names emended by change of stem seems necessary ; such names cannot advisedly be attributed retrospectively to an earlier author and date as for names merely changed in ending (paragraph 3.53). A rule for definition of priority which provides that the emended name shall be ranked exactly equivalent to the name it replaces may be judged advantageous, but a rule which calls for citing the emended name as though published by the author of the name it replaces and as though published on the date of the replaced name cannot be supported. An example cited in *B.Z.N.*, vol. 7, pt. 3, p. 73 is Erycinidae Swainson, 1827, based on *Erycina* Fabricius, 1807, as type ; later, when discovery was made that this genus is a junior homonym of *Erycina* Lamarck, 1805, the substitute name *Riodina* Westwood, 1851, was introduced with the same type species as for *Erycina* Fabricius ; the family name Riodinidae Grote, 1895, still later was published to replace the invalid Erycinidae Swainson. Because Riodinidae is not the oldest available name for the family, if it ranks from 1895, Mr. Hemming (*B.Z.N.*, vol. 7, pt. 3, pp. 77, 82) proposes that priority should be determined by the publication date of original designation of the family type (i.e. *Erycina* Fabricius=*Riodina* Westwood), and he suggests as a desired form of citation : Riodinidae (Erycinidae) Swainson, 1827 ; or alternatively : Riodinidae (Erycinidae emend. Grote, 1895) Swainson, 1827. Neither of these are acceptable because they actually comprise retrospective attribution of the

name Riodinidae to Swainson who never heard of the name, and less importantly because the suggested form of citation contravenes Art. 22 of the Rules, which provides that the author's name should immediately follow the scientific name without intervening punctuation, etc. Form of citation suggested for adoption in the *Treatise*, using the example just considered, is: Riodinidae Grote, 1895 (=emend. Erycinidae Swainson, 1827). As here used, " =emend." signifies equivalent (in having the same type genus and publication date for designation of the type genus) to the nomenclatorially emended older family name, and it means that the substitute name is ranked for priority purposes as exactly equal to the emended name. The author of the invalid replaced name is not (and should not be) cited as author of the new name.

3.63. *Names emended only by change of ending.* Such names are to be cited using author and date of the original publication, exactly as if the familial name had been then printed with the ending now adopted. Recommended *Treatise* form of citation is: Aulacoceratidae Mojsisovics, 1902 (as Aulacoceratinae).

3.64. *Superfamily names.* The author and date to be cited in connection with superfamily names are those referring to the oldest publication in which the superfamily is proposed as such (see paragraph 3.31). Almost invariably, the author of a superfamily assemblage differs from that of the type family on which it is based, and the date of original publication are not the same. Emendation of superfamily names considered in paragraph 3.53 does not affect author and date, but if the stem of the generic name on which the type family is based requires change, publication of a substitute family name (paragraph 3.52) does not automatically extend to changing the superfamily name; procedure in citing the altered superfamily name should be exactly analogous to that proposed in paragraph 3.62 for family names. Thus, if a superfamily Erycinoidea Smith, 1848, should have been published and no notice of it was taken by Grote when he introduced the new name Riodinidae for the type family in 1895, another author (say Holmes, 1900) might introduce an altered superfamily name, cited as: Riodinacea Holmes, 1900 (=emend. Erycinoidea Smith, 1848). Subsequent modification of the ending of the superfamily name would result in: Riodinicae Holmes, 1900 (=emend. Erycinoidea Smith, 1848).

3.65. *Family names.* The author and date to be cited in connection with family names are those referring to the oldest publication in which any genus included in the family is proposed as type of a familial assemblage (family, subfamily, or tribal category) and the stem of its name is used in forming the name of the familial unit. Thus, Mesopleuridae Jones, 1890, may be cited now on the basis of original publication as Mesopleurinae Jones, 1890, etc. (See paragraph 3.32.)

3.66. *Subfamily names.* The author and date to be cited in connection with subfamily names are those referring to the oldest publication in which any genus included in the subfamily is proposed as type of a familial assemblage. Two types of subfamily names call for notice.

3.661. *Nominotypical subfamily names.* Every family which is divided into subfamilies must have a nominotypical (*sensu stricto*) subfamily, which has as its type genus the same one which serves as type of the family. Because the name of the family is based on the generic name which among all included in the assemblage was first published as type of a familial category, this applies also to the nominotypical subfamily; also, the author and date of the nominotypical subfamily are invariably identical with those of the family, without reference to whether the author of the family or some subsequent author introduced subfamily divisions. Thus, the family Astrocoeniidae Koby, 1890, contains the subfamilies Astrocoeniinae Koby, 1890, and Pinacophyllinae Vaughan & Wells, 1943 (*not* Astrocoeniinae Felix, 1898). Not acceptable is the view held by some authors that a nominotypical subfamily may have neither author nor date, being cited simply as Astrocoeniinae *sensu stricto*, on the ground that this assemblage may comprise simply the residue of the family (including the type genus) when one or more subfamilies based on genera other than the type are set apart; every name recognised in zoological nomenclature was first published by some author on some date. Also, just as the nominotypical subgenus of a genus must be ascribed to the author who erected the genus and must bear the same date, a nominotypical subfamily cannot be attributed to an author other than the one who first selected the genus which serves family and nominotypical subfamily as type and it cannot bear a date subsequent to that of erecting the family.

3.662. *Other subfamily names.* Any genus included in a family, except that which serves the family and nominotypical subfamily as type, may constitute the type of another subfamily. The principle of priority demands, however, that if any genus included in a subfamily has previously been published as name-giver of a familial assemblage, the oldest available one must be recognised as type of the subfamily.

3.67. *Supertribal, tribal, and subtribal names.* The author and date to be cited in connection with supertribe, tribe, and subtribe names are those referring to the oldest publication in which any genus included in the assemblage is proposed as type of a familial category. The consideration affecting nominotypical subfamilies and other subfamilies apply in identical manner to tribal names.

3.7. *Examples of Procedure in Nomenclature of Familial Assemblages*

Following are a few examples, mainly based on data supplied in a memorandum by Mr. Leslie Bairstow on "Authors and Dates of Family and Subfamily Names." Conclusions are those of the *Treatise* Editor, presumed to accord with policy outlined in foregoing paragraphs.

3.71. *Family names based on Sepia.* Published initially as family names are: Sepiidea Leach, 1817; Sepiedea Lesueur, 1821; Sepiadae Owen, 1836; Sepidae d'Orbigny, 1837; Sepiidae Adams, 1858. Conclusion: citation should be Sepiidae Leach, 1817.

3.72. *Family names based on Octopus.* Published initially as family names are: Octopia Rafinesque, 1815; Octopoda Férussac, 1821; Octopidae d'Orbigny, 1837; Octopodidae d'Orbigny, 1838. The name Order Octopoda Leach, 1817, also was published. Conclusion: citation should be Octopodidae Férussac, 1821 (for stem is Octopod-, not Octop-).

3.73. *Family name based on Belosaepia.* Original publication of the generic name was *Belosaepia* Volz, 1830; the name *Belosepia* Volz, 1830 (published later in the year) is invalid for it is an objective junior homonym of *Belosaepia* (Art. 35 of Rules; see marginal note 82, Circ. 4, p. 18). The family name Belosepiidae Gill, 1871, is invalid because no valid generic name *Belosepia* exists for designation of the family type. No record is given by Bairstow as to publication of the correct family name, Belosaepiidae. If it is yet unpublished, a proper entry in the *Treatise* would be Belosaepiidae Bairstow, 1953 (=emend. Belosepiidae Gill, 1871).

3.74. *Family name based on Cirroteuthis.* Just like the case cited in 3.73. *Cirroteuthis* Eschricht, 1838, incorrectly changed to *Cirrhoteuthis* Möller, 1842 (objective junior synonym); family name published as Cirrhoteuthidae Keferstein, 1865. Assuming no other equivalent family name published, this might be recognised in *Treatise* as Cirroteuthidae Bairstow, 1952 (=emend. Cirrhoteuthidae Keferstein, 1865).

3.75. *Family name based on Loligo.* Incorrect family names based on *Loligo* are Loligoidea Lesueur, 1821, and Loligidae d'Orbigny, 1837; the correct name Loliginidae was first published by Gill in 1871. Citation in the *Treatise*: Loliginidae Gill, 1871 (=emend. Loligoidea Lesueur, 1821).

3.76. *Familial names based on Cypridina.* The genus *Cypridina* Milne Edwards, 1840, was chosen as type of a family called Cypridinadae by Baird, 1850, and published first as Cypridinidae by Brady, 1868; Dana in 1852 made first reference to a subfamily named Cypridininae, and Skogsberg in 1920 defined a superfamily called Cypridiniformes. Procedure outlined in this Circular leads to recognition of Superfamily Cypridinicae Skogsberg, 1920; Family Cypridinidae Baird, 1850; Subfamily Cypridininae Baird, 1850 (not Superfamily Cypridinicae Sylvester-Bradley, 1953; Family Cypridinidae Brady, 1868; Subfamily Cypridininae Dana, 1852).

3.77. *Family name based on Diplobelus.* The generic name *Diplobelus* Naef, 1926, is an objective junior synonym of *Diploconus* Zittel, 1868 [*non* Haeckel, 1860] but valid because of the unavailability of Zittel's designation of the genus. Therefore, Diploconidae Naef, 1922, must be replaced by Diplobelidae Naef, 1926. The suggested form of citation in the *Treatise* is: Diplobelidae Naef, 1926 (=emend. Diploconidae Naef, 1922).

3.78. *Family name based on Geopeltis.* The family name Geoteuthidae Naef, 1921, based on *Geoteuthis* Naef, 1921 [*non* Münster, 1843] must be replaced by Geopeltidae Altena, 1949, based on *Geopeltis* Altena, 1949, which is an objective junior synonym of Geoteuthis Naef. *Treatise* citation: Geopeltidae Altena, 1949 (=emend. Geoteuthidae Naef, 1921).

4. Nomenclatural Procedure for Suprafamilial Categories in Treatise

In choosing names for designation of suborders and higher taxonomic categories, *Treatise* authors are to use their own best judgment, no standard endings for these ranks having been adopted. It is desired, however, that the author and date of first publication of terms used shall be included in manuscript.

DOCUMENT 3/26

By JOSHUA L. BAILY, Jr.
(*San Diego, California, U.S.A.*)

Statement received on 24th June 1952

The name of a family should always be derived from that of its typical genus. The proposal of a family name should always be construed as a designation of the genus from which the family name has been derived as the typical genus of the family, and the designation of a genus as typical of a family should be construed as a proposal of a family name derived from that of the genus. Possibly some confusion may result temporarily from a strict application of such a rule, but the final result will be beneficial.

I know of only three instances of family names not derived from the name of their typical genera ; in each instance the family name has been rejected in favor of one formed from that of the typical genus and this step has met with universal approval. The first of these is the STREPOMATIDAE. When Tryon published his monograph of this family in the *Smithsonian Miscellaneous Collections* in 1873 he used the name *Pleuocera* instead of *Strepoma* because *Pleurocera* was first published, but he called the family STREPOMATIDAE. However, by common consent this name has been abandoned in favor of PLEUROCERIDAE. Incidentally, the last name is badly formed ; the name of the genus is not Latin but Greek, and the family name should be PLEUROCERATIDAE ; when it is decided to establish an *Official List of Family Names* and a corresponding *Official Index of Rejected and Invalid Family Names*, I shall request that these two names be placed on the appropriate lists to validate PLEUROCERATIDAE and reject PLEUIOCERIDAE.

The next family is the CYPRINIDAE, from its typical genus *Cyprina*. The rules require that the family name can be changed only when the name of the typical genus is changed. In this case *Cyprina* dates from Lamarck 1822. An earlier name for the same genus is *Arctica* Schumacher, 1817. In the English translation of Zittel's *Textbook of Paleontology* by Eastman this family has been renamed PLEUROPHORIDAE, changing not only the family name but also the genus from which the name is to be derived. I should think ARCTICIDAE would have been a preferable name.

The third family is the TETHYMELIBIDAE, named for two of its genera, *Tethys* and *Melibe*. At the time this family name was first devised, the name *Tethys* was an illegal one for this genus, which was properly called *Fimbria* ; recently the Commission has suppressed the name *Fimbria* and an earlier use of *Tethys*—and has validated the name *Tethys* for this genus. But since the family name TETHYMELIBIDAE goes back at least as far as the Cambridge Natural History of 1895, long before the validation of *Tethys*, it would seem to me that MELIBIDAE would be the preferable name for the family. I would request that a rule be adopted to make the family name agree with the generic name of the typical genus in every instance, and the designation of either a family name or a typical genus of a family be construed to include the other, and that the three families hereinabove discussed be known hereafter as PLEUROCERATIDAE, ARCTICIDAE, and MELIBIDAE.

Para. **16**

The name to be given to a family in cases where that family is formed by uniting two previously established families should be the older of the two family names concerned. Three alternatives are discussed in the *Bulletin*. One is that the name of the composite family should be derived from that of its oldest genus ; another is that the name of the family used by the first reviser should stand. These possibilities, after discussion, were rejected on adequate grounds. I am in complete sympathy with this argument. But the third possibility, which is recommended in the *Bulletin*, seems to me equally objectionable, because it contains the modifying phrase " each family for this purpose being identified by the type species of its type genus." I believe that the older of the two family names be accorded priority irrespective of whether a type species has ever designated for its type genus. I cannot recall any instance of the union of two families except where originally there was but one, subsequently divided, in which case the union of the two resultant families simply restores the original single family. In such a case the original name should be restored as well, but the ruling recommended in the *Bulletin* might make it necessary to use a new name and discard the original one. It might easily be that the younger of two family names might be derived from the first genus to have a type species designated, we would then have to use the younger of the two family names. This would unnecessarily complicate matters, since there is no similar requirement for a family formed in any other way.

Para. **34**

The use of the expression " tribe " for a group below the family level is to be deprecated. In ordinary parlance, a tribe is a group of families, such as a tribe of Indians, or the tribes of Israel. In zoological nomenclature the same usage has been established, and a new meaning for the term is not desirable. Also the statement that the termination " -INI " for tribes is commonly accepted is probably not correct. I do not recall ever having encountered a tribe having that termination. Some tribes take the ending " -GLOSSA," some " -SIDA," some " -MORPHA," and some " -GNATHA," according as to the anatomical feature upon which the tribe is based. I do not recommend any change in the present practice.

Para. **40**

The statement that the names of superfamilies generally have the termination " -OIDEA " surprises me. The only work that I can now recall in which this termination has been used is Hannibal's work on the fresh water Mollusca of California which he published in the *Proceedings of the Malacological Society of London*. Every other writer, as far as I know, uses the termination " -ACEA " for superfamilies. This is a practice which I would like to have perpetuated.

Para. **43**

There seems to be no good reason for establishing *Official Lists* for names of categories above the genus level. Species exist in nature ; rules governing the names of such are necessary, and *Official Lists* are helpful. Specific names include generic names, so that similar provisions are needed for them. Family names are based upon generic names, and rules may be needed, but I am dubious

as to value of an *Official List of Family Names*. In any case, families do not exist in nature and their names are not included in specific names. And names of higher categories are merely names of mental conceptions. To compile lists of official names of these conceptions is likely to get the commission into the business of legislating on matters that are not exclusively nomenclatorial, by perpetuating, not objects or names, but mental conceptions.

DOCUMENT 3/27

Statement of the views of the scientific staff of the
ROYAL ONTARIO MUSEUM OF ZOOLOGY AND PALAEONTOLOGY, TORONTO, CANADA

Enclosure to a letter dated 26th June 1952, from Dr. F. A. Urquhart, Director
(For the text of the above letter, see Document 1/39)

Family Names (Commission's Reference Z.N.(S.)357)

OUR PROPOSED SUGGESTION

One of the great difficulties facing systematic workers who are involved in university lectures is to plan an adequate course which will give the students some basic idea of the importance of Latin and Greek in the formation of our scientific words. We realise that a short course is most inadequate and leads to all sorts of difficulties. May we therefore make the following simple suggestion ?

If such is possible (and we realise the difficulties that would be involved) we would propose two alternatives :—

(a) that a set of rules be arranged by a committee, set up for this purpose, which rules will apply to the formation of scientific names throughout the field of systematic zoology. These rules would perhaps have a part bearing upon Greek and Latin and in part on modern usage. Perhaps a scheme similar to that proposed by Dr. Blackwelder might be formulated ;

(b) that the ending of all names for genera, species and subspecies be made constant. We realise that this would be a very drastic change and perhaps an oversimplification. However, in view of the fact that we now have uniform endings for the names for subfamilies, families and tribes, we feel justified in suggesting that we go still further and have uniform endings for names for the genus, species and subspecies. After considerable discussion, we came to the conclusion that the ending of the name for a genus, species or subspecies added nothing to the word itself except considerable difficulties in ascertaining the gender of the word concerned. We were also of the unanimous opinion that our major interest was in the object and not in the word applied to the object. Whether the word in its derivation is masculine or feminine we feel adds absolutely nothing to whether the animal concerned is masculine or feminine. In view of the fact that the vast majority of our specific names, particularly in invertebrate zoology, end in either

" us," " a " or " um " we proffer the following suggestion: that all generic names might end in "us," that all specific names might end in " a " and subspecific names in " um."

This discussion may have only a partial bearing upon the problem of family names discussed under the Commission's reference number Z.N.(S.)357. However, it is discussed at this point since it has a definite bearing on family and subfamily names.

We unanimously agree with your conclusion given at the end of paragraph **11,** on page 71 of volume 7, part 3.

We unanimously agree that the general rule would be that a name is to be treated as having been published as a family name only when the term used for this purpose was formed with the termination " -IDAE."

We unanimously agree that this scheme might make it clear that a family name is not subject to change when the nominal genus upon which it is based is subjectively identified with another nominal genus having an older available name, and to make this subject to the proviso that, where specialists in the group concerned desire that in such a case the name of the family concerned should be changed, it shall be open to them to make application to the Commission accordingly.

We unanimously agree that, of the two methods discussed by you for determining which of two or more competing family names should be accepted in preference to the others, we prefer the mechanism of the first reviser, as described by you in paragraph **19,** page 77 of volume 7, part 3.

We could not appreciate the force of the objections advanced by Professor Jeannel.* It is our unanimous opinion that the ending of the word is not the significant part of the word in so far as the particular object is concerned. Whether the word is masculine or feminine is, in so far as we are concerned, of very little importance.

We unanimously agree with your recommendation as given at the bottom of paragraph **32,** page 85 of volume 7, part 3.

We unanimously agree that the name of a tribe is to be formed by the addition of the termination " -INI " to the stem of the name of the type genus of the tribe concerned and that the names of tribes are to be subject to the same rules as, and are to be co-ordinate with, the names of families and subfamilies.

We unanimously agreed that we have not found it convenient to recognise the category " supergenus." However, we are well aware of its present usage and the degree of variation in its application. We would however be agreeable to any conclusions which may be presented to you from those who have consistently used the supergenus and wish to have it included in the rules.

We unanimously agree that the revised scheme should contain a provision prescribing that the name of a superfamily is to be formed by adding the termination " -OIDEA " to the stem of the name of the type genus of one of the included families. Further, that the names of superfamilies shall be co-ordinate with the names of families and that such names shall rank for priority by

*See Document 3/9.

reference to the dates on which they were severally published as such, and not by reference to the relative priority of the names of the families comprised in the superfamily concerned.

We unanimously agree that a strong recommendation should be submitted for the establishment of an *Official List of Family Names in Zoology* and that such should form a part of the revised scheme for the regulation of family names.

F. A. Urquhart,
Director,
Royal Ontario Museum of
Zoology and Palaeontology,
University of Toronto.

DOCUMENT 3/28

By HENNING LEMCHE
(*Universitetets Zoologisk Museum, Copenhagen*)

Enclosure to a letter dated 30th June 1953

Remarks to the Proposals Concerning Family Names in Zoology (Z.N.(S.)359)

General Remarks.—The purpose of our Commission is to decide in questions that cannot be regulated in the normal way by common agreement among authors. Thus, we are to leave to the scientists themselves to run their science as long as they are able to do it without controversies, and we help them therein by giving them the general advice through the *Règles*. The point, however, is that we have no means by which we can force anybody to do any special thing. We must never forget that we cannot make laws, but that our *Règles* are in fact nothing but strong recommandations. If we fail to make the *Règles* useful, there will come a day when the scientists will lose their patience and break the *Règles* intentionally, and then we will have that anarchy in nomenclature, which we all aim at escaping.

Therefore, it is constantly to be remembered that any single superfluous word in the *Règles* is most harmful, as it adds to the difficulties involved in their interpretation. *The* Règles *must be clear, short and easy to handle.* If there is a topic where the free action of the scientists does not do any serious harm, we should make no *Règles* that could be broken. This is the case in the problem about a detailed ruling on the names of families, etc., as I will try to show.

There ought to be a regulation of some kind that no commonly accepted name is to be altered for nomenclatorial reasons except after special decision by the Commission. This would place the burden of trouble on the shoulders of the man who wants to change, and not on those people who want to work with unchanged tools.

I would propose that, instead of all the detailed provisions in nearly every paragraph that the Commission is to be asked for exceptions, there should be incorporated in the *Règles* a very short sentence saying that : "*In every single case where it furthers the stability of nomenclature and is in the interest of the zoological science to make exceptions from the* Règles, *the Commission has the power to make that decision.*"

Probably, this sentence could be added to Article 1.

Special Notes

Para. 6.—It is a good idea to have a schedule explaining how to make the Latinized names properly. Such a schedule should be no part of the *Règles* themselves, but merely recommandations. Then, any failure to adhere to this advice could be no source of trouble, and we can be sure that some such failures will show up even in spite of the best schedule. However, the schedule should not be too intricate in order to be complete. Better leave the minor points out of consideration. Should some more important error

appear, the Commission could be asked to rule. This would give the Commission much less trouble than if detailed *Règles* are established and—because of their complexity—constantly broken inadvertently.

Paras. 7-10.—Is there any reason why incorrectly formed family names should not be treated as the spelling mistakes they in fact are ? The *Règles* concerning spelling mistakes could easily be made to comprise these cases. I am against any special ruling in this matter.

Instead of introducing " laws " it is better to make recommandations. Then, people will have a " guide " as to what to do properly, without possibility of endless debates arising out of minor failures. The difficulties in nomenclature up till now have most often been that the *Règles* were too strict.

Para. 11.—The first thing to do is to find out whether or not it is at all advisable to establish any *Official List of Family Names.*

(1) If we are to protect the family names in the same way as generic and specific names, it is absolutely logical that we also make the proposed list. The problem is, however, whether it will be useful to us to be logical. Logic can be carried to such an extent that it hampers practical work and gives birth to bureaucratism. We must avoid that.

(2) Till now, we know very little concerning the effects of the lists of generic and specific (trivial) names arranged for earlier. So, it is a little early to judge the usefulness of such lists and we are to be a little reluctant to establish new ones if not absolutely necessary. Every complication is to be avoided.

(3) Regarding priority, there are the same problems about genera and about families, but the weight of the arguments is different. Closely related nominal genera may or may not be identical in practice, but, still, if we change from one nominal genus to another with a different type species we also change the concept of the genus a little, because it becomes a new " centre." We cannot avoid this difficulty on the generic level if we want stability in the generic names. So, we take a minor trouble to avoid a major one. It is, however, no absolute consequence that the same reasoning is valid on the family level. If we change from one nominal genus to another, we change the name totally, and repeated changing will mean that names show up and disappear totally all the time. This is the major trouble. On the family level, however, it is the ending, only, that is new. The genus on which the family name is based will be in the family all the time, and so it is much easier to trace the meaning of a new family name. Therefore, the same problem is a minor one, only, on the family level.

(4) Consequently, what is the basic problem in our case is to find out whether the lack of uniformity in family names is such a trouble that it is preferable to introduce a ruling similar to that concerning genera. Or, will it be preferable to leave every author free to choose the centre of his family-concepts around any genus he might prefer ?

I have not myself been working enough on the family-level in taxonomy to be able to answer this question properly, but I find a most lucid discussion thereof in the remarks of the Secretary in the paragraph discussed here. He

states that "there is no doubt that authors should be free to choose as the type genus for a family whatever genus he may regard as the most representative. . . ."

Consequently, families established on different type genera cannot have "priority" over each other. Then, there can be only historical reasons to place authors' names behind a given family name (see below) and *an Official List of Family Names* becomes illogical as it would say only that a family based on a certain type genus is to be called so and so—which is exactly what is told us in the Article 4.

Para. 12.—See the remarks to para. 25.

Para. 13.—The problem disappears if *no* priority status *is* introduced.

Paras. 14-15.—The clear thing to do is to state that family names are to be derived from their typical genus. As long as authors do not agree in the taxonomic question of what is the better type genus to choose, stability cannot be attained by any nomenclatorial means whatsoever. So, again the problem disappears if *no* priority status is given to family names. Exceptional cases can be handled by the Commission, but I think they will be very few. They will not by far run to the number of cases that will arise if we introduce the idea of priority here.

Paras. 16-20.—All these problems disappear if *no* priority status is introduced.

Paras. 21-24.—Here, we better have to refer to a general rule that the Commission has the power to regulate in every single case where stability is threatened by strictly following the *Règles*. (See the General Remarks.)

Para. 25.—In the case of species or genera, the authors' name and date is necessary because we have to know whether the name in question applies to some given species (genus) or a totally different one used for something else by some other author at another time. The purpose is not to carry any record on the history of zoology. It is absolutely necessary to remember that nomenclature is a practical tool—to be used for working purposes. If nomenclature is made a museum exhibition for the history of zoology, it will become a burden and not a help to taxonomy.

Turning now to the question of how to treat the family names, all that is left of the reasons for citing authors' names, etc., is the historical one. The family is bound to be defined by its typical genus from which the family name is derived. Later authors, if using the same type genus, are bound to use the same family name, even if the width they give their family concept is very different from that which the original author held. So the original author will be credited for having established a family concept that is totally different from what were his intentions when he actually made it. So the historical record that is kept in citing the authors is a false record.

According to the discussion to para. 11 by the Secretary, authors using other genera as types must be regarded as using other nominal family concepts. Priority does not come in here at all. The arising practice of citing authors in connection with family names is simply a needless taking over of something

useful in other cases (genera) to a sphere (families) where it is of no—or rather negative—value. So, we will save ourselves from endless troubles if we do not rule anything concerning priority in family names, and this is the reason for my proposed parenthesis to the first sentence in Article 4.

Moreover, the citation of authors in families could even be made useful if the meaning of it was changed so that if an author is cited, this would mean " sensu author date." In order to avoid misunderstandings, however, it seems preferable, then, always to use the word " sensu." We should, at least, not prevent a development along such a line by introducing a procedure that is of no use.

Paras. 26-28.—The problems disappear if no priority status is introduced. (A single remark to para. 26 : why should we make nomenclatorial rulings about how to tell the history of zoology ?)

Para. 29.—I fail to see that any regulation is necessary on the family level, as the problem is solved already through the rulings concerning generic names. In the few cases where similarity in family names arises out of derivations from different generic names, the Commission would have the power to decide from the general rule of the powers of the Commission in every single case where confusion is threatening.

Paras. 30-32.—I agree.

Para. 33.—The problem disappears if *no* priority status is introduced.

Para. 34.—Although I agree that the category of " tribe " is sufficiently well established to be regulated in some way, I disagree in the theoretical considerations. Before Darwin, the animal system was regarded as something formal. Then we have had a period where the theory of descendence has been a governing factor in zoology. Modern experience, however, begins to show that it may always be impossible to work out detailed " true " relationships, because the tracks through which genes are inherited through the ages are so extremely interwoven. So, in ruling as if the idea of phylogenetical relationships were the only one possible in zoology, we might cause trouble the day this idea is no more held.

Para. 35.—I fail to see the need of special names in such cases. Then, we could continue endlessly. Authors wanting to indicate groups in between our better recognised levels could easily do so by using symbols (Roman and Arabic figures, capital letters, etc.). We do not want over-systematising in nomenclature.

Para. 36.—People that do not know what is a tribe do not need any advice through our *Règles*—they will have to ask their professor. We have no definition, either, of the concept of a family, or a genus, or even a species. All this is outside our scope. We must avoid superfluous matter in order to keep our *Règles* clear and short.

Paras. 37-38.—I cannot find any necessity for ruling about supergenera. It is not appropriate to introduce the practice that if some author invents a new category, we will have to rule about it and give it a definite status in nomenclature (compare the remarks to para. 35).

Para. 39.—I agree.

Para. 40.—Why not simply use the termination idea ? The superfamily is very close to a family, and the category is not very widely used ; why not, therefore, use a termination similar to that of the family ? Then, we could possibly spare the termination -oidea for—say—orders.

(As discussed in detail elsewhere, I do not agree that we are to accept as final the diversity of name endings in orders.)

Para. 41.—I fail to see the reason for ruling about superfamily names in any other way than stating that they are to be co-ordinate with the name of their type family. As in family names, the question of priority cannot come up at all. Such rulings would be taxonomical not any of nomenclature. Don't paralyse taxonomy in order to avoid some few inco-ordinate jerks.

From all the above considerations, it seems to me that we could reduce the number of necessary *Règles* to the few given below :—

Article 4

The name of a superfamily is formed by adding -idea (or -oidea), the name of a family by adding the ending -idae, the name of a subfamily by adding -inae, and that of a tribe by adding -ini to the stem of the name of its type genus.

(N.B.—Authors' names are no part of family, etc., names.)

The name of the nominotypical family (subfamily, tribe) is to be derived from the same generic name as that of the superfamily (family, subfamily).

No more than one family, etc., can bear the same name. In cases of homonymy, one of the names is to be changed by the International Commission.

(Recommandation : Authors are urgently requested to choose their type genera in such a way that homonymy will not appear.)

(Note : The family concept is the only one of those cited that is obligatory.)

Article 5

The name of a superfamily, family, subfamily or tribe is to be changed whenever its typical genus is changed.

Incorrectly formed family, etc., names are to be treated according to the general rulings concerning spelling mistakes.

(Recommandation : If the error is small and harmless and stability has been attained, authors should not be too interested in changing, but better wait until the fifty years' limit has been reached and the name thus becomes protected*.)

If the name of a nominal genus for some family, etc., is changed or—for the reason that it is by some other author regarded as a junior homonym to some other nominal genus—the name of the family, etc., is to be changed accordingly except in every single case where the Commission decides that it should be otherwise.

*This Recommendation is only to be added in the case that a law of prescription is introduced making invalid all names that have not been used for fifty years.

DOCUMENT 3/29

By A. MYRA KEEN and SIEMON W. MULLER
(*Stanford University, Stanford, California, U.S.A.*)

Enclosure to a letter dated 1st July 1952

Comments on the Proposed Clarification, Amendment and Expansion of the Provisions in the "Règles" Relating to the Formation of the Names of Families

So many complications arise when an attempt is made to extend the *Règles* to cover categories higher than the genus that the task seems to us fruitless, adding a burden of bibliographic research to the work of the systematist that serves no really useful purpose. The concept of a family is a fluid one, changing both with time and with the viewpoint of the worker. The labor involved in establishing who in the past first used the currently acceptable combination of letters, or even—if one broadens the base—of determining who first used the word "family" in connection with a given group, seems to us a sterile and pedantic waste of time. The only point we would see in using the name and date of an author would be to indicate that the family as cited in a report includes the genera grouped therein by that author.

As to the collateral question (*Bull. zool. Nomencl.* **7** : 89, paragraph 40) concerning name endings for superfamilies: The termination "-ACEA" is, in Mollusca, more frequently used than "-OIDEA." We feel that either of these is preferable to the termination "-ICAE" favored by some workers.

DOCUMENT 3/30

Statement of the views of the
COMMITTEE ON NOMENCLATURE OF THE AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Enclosure to a letter dated 10th July 1952

(For an extract from the above letter, see Document 1/41 (p. 71))

Z.N.(S.)357. Rules for Naming Families and Suprageneric Categories of Lower Rank : Family names are to be formed by adding " -IDAE " to the stem of the name of the type genus. Names will date from the first author to use the name (regardless of its ending) in a suprageneric sense, not merely as a plural reference to the members of some particular genus. The type genus is not to be changed and is not necessarily the oldest genus or generic name in the family. Family names may be changed by the Commission in case of homonymy. Family names are not to be changed if the name of the type genus is changed (requiring repeal of Article 5) ; if necessary to avoid confusion, the Commission should place the generic name on the *Official List.*

The proposed necessity for citing author and date for family names may be questioned. In how many groups is this done ? The proposal that the priority of family names should be established on the basis of the family name whose type genus first had its type species elevated to that position is highly objectionable. It should, as a rule, be on strict priority of family names, with all cases to be decided by the Commission when undue confusion would be entailed by change.

Subfamily names (" -INAE ") and tribes (" -INI ") are co-ordinate with families as to formation of names, priorities, etc. Supergenera are of questionable status and of doubtful value as standard groups requiring standard endings. Rather, they are informal groupings of related genera and should not be standardized. They would, in any case, be co-ordinate with genera. We fail to see how this would occasion displacement of established generic names by downgrading, as claimed, since their names would be based on the names of the type genera.

Superfamilies shall be co-ordinate with families (oidea).

An *Official List of Family Names* is recommended with also an *Official Index of Rejected and Invalid Family Names.*

A.M.N.H. Committee on Nomenclature,
(Signed) Edwin H. Colbert,
John T. Nichols,
Ernst Mayr,
George H. H. Tate,
John T. Zimmer (Chairman).

DOCUMENT 3/31

Statement submitted on behalf of the
NOMENCLATURE DISCUSSION GROUP, WASHINGTON, D.C.

Enclosure to a letter dated 16th July 1952, from
Dr. R. E. BLACKWELDER, Secretary

Family Names (Z.N.(S.)357)

No attempt was made to cover all points under this subject, but after considerable discussion at an open meeting, the membership was canvassed on several particularly important and potentially controversial points. It appears that interest in the subject is especially high in the field of entomology, for twenty-one of the twenty-seven ballots received were from entomologists.

(1) The vote was unanimous (27 to 0) in *disapproving* the proposal to date family names from the time of their first publication with an " -IDAE " termination, and in *approving* instead the dating of names from their first proposal as a supergeneric category based on an included genus.

(2) The vote was overwhelming (25 to 1, with one not voting) in *disapproving* the dating of competing family names from the respective dates on which the type species of their type genera were fixed, and preferring instead the respective dates of proposal as supergeneric categories, as approved in the first question above.

(3) With reference to the status of family names when the name of the type genus of the family is found to be a synonym, opinion was much more divided, although majorities were recorded in each case :—

(*a*) By a vote of 17 to 8 (two not voting), *approval* was registered for not changing family names when the familiotype comes to be regarded as a junior subjective synonym.

(*b*) By a vote of 16 to 9 (two not voting), *disapproval* was registered on the proposal to change family names when the familiotype is found to be a junior objective synonym.

With one exception, the majorities on these two questions were composed of the same people. In other words, sixteen of the twenty-five voting taxonomists approved basing a family name on a generic name even after it falls into synonymy, whether subjective or objective. One would not change in cases of subjective synonymy, but would for objective. Eight would change the family name in both types of case.

DOCUMENT 3/32

By C. W. WRIGHT
(*London*)

Enclosure to a letter dated 18th July 1952

Comments on Questions Relating to the Regulation of Family Names Raised in Part 3 of Volume 7 of the "Bulletin of Zoological Nomenclature"

Para. **6**

In the light of experience of authors who are otherwise good nomenclaturists, I think that a Schedule is essential!

Para. **14**

I agree that priority must date from the concept, the name following the generic name.

Para. **15**

I feel strongly that the *name* of a family is so closely involved with the name of the type genus that the *rule* must be that it changes whenever the name of the type genus is validly changed but that *where specialists desire the family name NOT to be changed application may be made to the Commission.* The principle should surely be that in the permanent rules logic should be served, with provision for the meeting of expediency by the granting of exceptions by the Commission. Otherwise the Rules will become a hotch potch.

Para. **18**

I agree emphatically.

Para. **20**

I feel strongly that exceptions should be granted only on the same basis and to the same extent as in the case of generic names. After all in most phyla the number of actual users of family names in any generation amount to only a few dozens and, if the *Official List* policy works, the problem will be rapidly reduced in size.

Paras. **21-24**

The solution lies in (*b*) of para. 24. The desirability of retaining a family name should be made explicitly one of the factors to be taken into account when applications relating to generic names are considered. I am most averse to any provision that would in any circumstances allow the retention of family names with no nominotypical genus, as this is flatly contradictory to the whole basis of familial nomenclature in the Rules.

Para. **26**

I agree but suggest that the full method of citation should be: "RIODINIDAE (Grote, 1895: ERYCINIDAE) Swainson, 1827" which gives the chronology in the right order.

Para. **32**

I agree but consider that provision should be made for method of citation —in my view, and I believe yours, this is correctly " A-IDAE (A-INAE) " with NO author and date for the nominotypical subcategory.

Para. **40**

I disagree with the suggestion of " -OIDEA "—see Arkell's comments with which I agree. Surely if superfamilies go with families, one essential is that they should end in " -AE " ; *Treatise of Invert. Pal.* at one time favoured " -ACEAE " but now apparently has gone over to " -ICAE," which has the merit of corresponding well with " -IDAE " and " -INAE."

DOCUMENT 3/33

By E. RAYMOND HALL
(*University of Kansas, Department of Zoology, Lawrence, Kansas, U.S.A.*)

Enclosure to a letter dated 22nd July 1952

Use on basis of priority ; probably we should not interpret priority to depend on the ending " -IDAE " for the family names.

DOCUMENT 3/34

Statement submitted on behalf of the NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY

Letter, dated 23rd July 1952, with enclosures, from Dr. W. I. FOLLETT, Chairman

Families (Z.N.(S.)357)

The Nomenclature Committee of the Society of Systematic Zoology submits the following recommendations on the naming of families, superfamilies, and suprageneric categories of lower rank :—

(1) Any genus, not necessarily the oldest included, may be the familiotype.

(2) A family name should date from its first publication as a suprageneric group name based on an included genus, regardless of the ending employed and regardless of what the author called his suprageneric category (e.g. family, stirps, etc.).

(3) The name of a superfamily should be formed by adding " -OIDEA " to the stem of the name of one of the included families.

On the remaining aspects of this subject concerning which you have requested advice, the varying shades of opinion of the committee members are expressed by the following (copies of which are enclosed in duplicate) :—

(1) Mr. Curtis W. Sabrosky's summary, discussion, and recommendations (Appendix 1).

(2) Dr. J. C. Bradley's remarks upon Mr. Sabrosky's paper. Dr. Bradley, as you doubtless realise, is not a member of this committee (Appendix 2).

(3) Mr. Sabrosky's comments upon Dr. Bradley's remarks (Appendix 3).

(4) The comments received from each member of the committee who has expressed his views. You will note that the comments of one member are submitted on a copy of Mr. Sabrosky's paper, and are distinguished thereon by *underlining** (Appendix 4).

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*The comments which were distinguished in the typescript by underlining are here printed in bold type.

APPENDIX 1 TO DR. W. I. FOLLETT'S LETTER OF 23RD JULY 1952

Enclosure 1 to Appendix 1 to Dr. Follett's letter noted above

SUMMARY BY MR. CURTIS W. SABROSKY OF THE SUGGESTIONS PUT FORWARD BY MR. FRANCIS HEMMING IN VOL. 7 OF THE "BULLETIN OF ZOOLOGICAL NOMENCLATURE" AND COMMENTS ON THOSE SUGGESTIONS BY PROFESSOR ROBERT L. USINGER

(Note : In the typescript submitted to the International Commission, Professor Usinger's comments were distinguished by being underlined. In the present version those comments are distinguished by being printed in "bold" type.)

Z.N.(S.)357 : Names of Families and Co-ordinate Categories ("Bull. zool. Nomencl." 7 (pt. 3) : 61-94 ; March 15, 1952)

(Summary by Curtis W. Sabrosky)

Summary of Mr. Hemming's Proposals (Arabic numerals below correspond to paragraph numbers in the *Bull. zool. Nomencl.* For brevity, I use "familiotype" (type genus of a family). The expression "etc., etc." takes the place of the details of application to the Comm., public notice, etc.)

(*a*) NAMES OF FAMILIES AND SUBFAMILIES

STEM :

4-6 : The key feature of Article 4 (the "stem") "seems inevitable" and "must be maintained." It is proposed to add a Schedule of criteria for determining the stem of Latin and Greek nouns. (A modification of the Paris action (B.Z.N. 4 : 246), back toward classicism.) **O.K. à la Grensted and Sabrosky.**

8-9 : For family names published prior to the effective date of the Schedule on stems :—

(x) If they have never been emended "on etymological or philological grounds," no emendation will be permitted after that date (whether name is correctly or incorrectly formed).

(xx) If an emendation already exists, the Commission can direct, upon application, etc., etc., that it be accepted.

O.K.

10 : For new family names, after the effective date, the Schedule rules will be mandatory, and incorrectly formed names will be "subject to automatic emendation." **O.K.**

TYPE :

11 : Any genus, not necessarily the oldest included, may be the familiotype (*Opinions* 133 and 141 reaffirmed). **O.K.**

SUBFAMILY NAMES :

30-32 : To be governed by the same rules as family names, except that they shall end in "-INAE," and that the subfamily containing the type genus of the family shall be formed on the same stem as the family name. **O.K.**

33 : Family and subfamily names are co-ordinate in value (comparable to provisions in Articles 6-7 and 11-12 of the Code). **O.K.**

DATE OF PUBLICATION :

12-13 : A family name is to be considered established only when published with the termination " -IDAE," except that for cases before 1850, the Commission may direct otherwise, on application, etc., etc. (See also paragraphs 25 and 14.) **Agree with Sabrosky.**

AUTHOR :

25 : The author of a family name is the one who first published it (presumably in line with 12-13), save that a substitute name (14) should be attributed to the author and date of the original name which is replaced. **See Sabrosky.**

CITATION :

26 : Comparable to Article 22 of the Code, with special provision for recording substitute names (14, 25). **O.K.**

CHANGE OF NAME OF FAMILIOTYPE :

14 : In cases of generic homonymy, the resulting substitute family name is to be based on the substitute generic name, and is to be back-dated to the original proposal of the family. **O.K. but dual author and date.**

29 : Rules for determining generic homonymy, being fundamental to generic names and hence also pertinent to the question of family names, should apply also to family names based on those generic names (cf. B.Z.N. **4** : 130-131, 161-162, 243). **O.K.**

15 : If the familiotype becomes a *junior subjective synonym*, the family name should not be changed, unless specialists apply to the Comm., etc., etc. **O.K.**

21-24 : Two items are linked as being one nomenclatorial problem : (x) Family names not based on an included genus, and (xx) the proposal by some zoologists that family names should not be changed when the familiotype is found to be a junior objective synonym. " In each of these cases the *only nomenclatorial issue* is whether circumstances are *ever so exceptional* that it is desirable to waive the requirement that a family name must be based upon the oldest objectively available name for its type genus." (Italics mine.) Mr. Hemming believes that zoologists will wish a provision for exceptions, but he also believes that the general feeling is that exceptions should be made " only on very rare occasions, and where the need for maintaining stability for the name of the family concerned is exceptionally strong." He believes it an " extremely great " gain to be able to see from the family name what is the familiotype. He suggests, therefore, that exceptions should be rare, and should be limited to family names established " prior to (say) 1850." **? No.**

FAMILY NAMES IN COMPETITION :

16-19 : In cases of the union of two or more families, the choice of name for the combined family should be made by priority rather than by a first-reviser method. Mr. Hemming prefers a rule that competing family

names " take precedence as between one another as from the date on which the type species of the type genera concerned were elevated to that position." He chooses this expression, rather than the dates of publication, " in order to ensure that the concept of a given family should retain its original priority, even if for nomenclatorial reasons the actual family name had at some time (to) be changed, the family retaining its identity however by keeping unchanged the type species of its type genus." **Yes.**

20 : The Commission may, on application, etc., etc., direct the use of a junior family name over a senior where the former is best known and most widely used. **Of course.**

27-28 : Homonymy in family names derived from distinctly different generic names : If it occurs, the junior homonym is to be rejected ; but specialists may apply for a ruling on which names shall be used, as in the case of Meropidae-Meropeidae (*Opinion* 140). **O.K.**

(*b*) NAMES OF UNITS BETWEEN SUBFAMILY AND GENUS

34-38 : The Rules should be extended to include (x) names between tribe and subfamily (to be formed like subfamily names), (xx) names of tribes (or equivalent units, whatever called) (to be formed with the ending " -INI "), and (xxx) names of supergenera (suggestions wanted on the termination to be adopted). All names are to be co-ordinate with family and subfamily names, save that supergeneric names " should rank for priority *inter se* by reference to the dates on which they were respectively published as such." **O.K. but leave out " supergenera." These belong in generic group—if necessary.**

(*c*) NAMES OF SUPERFAMILIES

39-42 : As in section (*b*) above, it is believed desirable to extend the Rules to cover these names. It is proposed to form such a name by adding " -OIDEA " to the stem of the name of one of the included families. The names will be co-ordinate with those of families, save that they shall rank for priority *inter se* from date of publication as superfamilies. **O.K.**

(*d*) OFFICIAL LIST AND OFFICIAL INDEX

43-48 : As has been done for generic names, it is proposed to add Schedules to the Rules to contain an " Official List of Family Names in Zoology " and an " Official Index of Rejected and Invalid Family Names in Zoology." The former is to include also subfamily and tribal names, and with provision for special entry of superfamily and supergeneric names if the qualifications noted above in (*b*) and (*c*) are accepted. **O.K. with exception of supergenera.**

(*e*) ADVICE SOUGHT

49 : Specialists are asked for their views on the problems, especially on the suggestions made for solution, and also for any additional aspects that should be covered. Statements are requested by not later than 31st July 1952.

Enclosure 2 to Appendix 1 to Dr. Follett's letter of 23rd July 1952

MR. CURTIS W. SABROSKY'S DISCUSSIONS OF THE SUGGESTIONS IN RELATION TO THE NAMING OF FAMILIES PUT FORWARD BY MR. FRANCIS HEMMING IN VOL. 7 OF THE "BULLETIN OF ZOOLOGICAL NOMENCLATURE," WITH MR. SABROSKY'S RECOMMENDATIONS IN REGARD TO THE FOREGOING MATTERS AND PROFESSOR ROBERT L. USINGER'S COMMENTS ON MR. SABROSKY'S RECOMMENDATIONS

(Note : As in the case to Enclosure 1 to Appendix 1, Professor Usinger's underlined comments are distinguished by being printed in " bold " type.)

FAMILIES ; Z.N.(S.)357. DISCUSSION AND RECOMMENDATIONS

By Curtis W. Sabrosky

Mr. Hemming's proposals have considerable merit. It is my opinion, however, that if the final proposal should treat some parts of the problem in the ways suggested, the solution as a whole will seem cumbersome, involved, and too dependent on the Commission.

One general simplification which I recommend is to make a positive statement of all provisions, and to eliminate the numerous explanations that specialists can apply to the Commission if they wish exceptions to be made. All of these can be taken care of by one final statement that unusual cases can be presented to the Commission with full particulars. **Good.**

It should be realised that the establishment of rules for family names based on the priority of their proposal, and even more important the extension of such rules on a co-ordinate basis to all categories between supergenus and superfamily, will impose grave problems and responsibilities for the future. Very few, if any, groups are adequately catalogued in the matter of suprageneric categories, and the larger the group, the more complex the problem. An across-the-board application of any rule will cause upsets of some familiar names. I have often wondered if we might not be farther ahead to organise international committees in the various phyla and orders, to review the literature, to follow some rules in general, to agree on arbitrary solutions for exceptional cases, and finally to produce a list of family names with their familiotypes and type species. It was such a programme as this which our Committee started some years ago, but which has been dormant for some time. It is also comparable to the programme on " The Generic Names of British Insects " being carried out in England, though that project is limited along national lines. It is perhaps still not too late to recommend such a programme on an international scale.

As a basis for discussion by our Committee, I wish to submit the following recommendations, arranged in the order of my summary :—

(*a*) NAMES OF FAMILIES AND SUBFAMILIES

STEM :

4-6 : Approve, but with two suggestions : (x) Recommend that the

Schedule of criteria follow the very reasonable conclusions of Grensted (1948. *Ent. mon. Mag.* **84** : 280-281) in interpreting certain nouns as Latinized and thus with a Latin rather than a Greek stem (e.g. ACROCER-IDAE rather than ACROCERAT-IDAE, based on *Acrocera :* nouns with Greek endings, such as *Rhinoceros*, would continue to follow a Greek stem, thus RHINOCEROT-IDAE. (xx) Recommend that the Schedule include a general statement for nouns that are not Latin or Greek, based on a common-sense use of what appears to be the natural or obvious stem, or the use in some cases of the entire generic name as the stem. **O.K. incl. suggestions.**

8-9 : I recommend a vote to omit these sections entirely, as a contribution to making the rules more simple and direct. Any changes that would be required by general enforcement of the rules on stems will, as far as I am aware, be of such minor nature that we should not bother about them. Examples : Changing PIERIDAE and SEPSIDAE to PIERIDIDAE and SEPSIDIDAE is not very earth-shaking. In fact, if such cases are the *raison d'etre* of 8-9, I would prefer to see the Schedule of criteria in 4-6 rule arbitrarily that all family names based on " -IS " nouns should be formed simply and directly by dropping the " -IS " and adding " -IDAE," thus PIERIDAE, EMPIDAE, etc. **O.K.**

10 : If 8-9 is left in, this paragraph is all right, but in line with my recommendation to omit 8-9, I recommend that the mandatory and automatic emendation features be made applicable to all family names. **O.K.**

TYPE :

11 : Approve, but I suggest that it would be appropriate to add here the provision, which Mr. Hemming has elsewhere, that the originally established *familiotype* is not subject to change, though its *name* may sometimes have to be changed. (Cf. my discussion of paragraphs 25 and 14.) **O.K.**

SUBFAMILY NAMES :

30-33 : Approve. **O.K.**

DATE OF PUBLICATION :

12-13 : Disapprove. Instead, I recommend supporting a rule that family names date from their first publication as a suprageneric group name based on an included genus, regardless of the ending employed and regardless of what the author called his suprageneric category (family, stirps, or etc.). This rule will avoid attempting to decide on a date before which some names may be validated on other than " -IDAE " endings (1850 suggested by Mr. Hemming), and will avoid the need for and the trouble with applications to save those old names. Actually, many of the old names which used other endings, before the " -IDAE " ending was widely and quickly accepted, are the old familiar families which everyone will wish to preserve anyway ; e.g. TIPULARIAE Latreille, CULICIDES Billberg, TABANII Latreille, etc. Example of the difficulties that may arise by insistence on the " -IDAE " form : The family name of the moth flies dates from PSYCHODITES Newman 1834, or (if my present information is complete) from PSYCHODIDAE Bigot 1854. But if one dates that only from the " -IDAE " ending in

1854, then the oldest name for the family is actually FLEBOTOMIDAE Rondani 1840 (emended PHLEBOTOMIDAE Walker 1851). **O.K. I agree with Sabrosky.**

One small additional point: Mr. Hemming mentions vernacular names, especially as used by French authors. I see no reason why these might not be accepted for the older names, provided, however, that it is clear that they are *group* names (suprageneric) and not merely a way of referring in the plural to the species of one genus.

AUTHOR:

25 : Approved except for the "saving clause." On the latter, I see no advantage to be gained by citing a substitute name by the author and date of the name which is replaced, if the Rules somewhere make clear (probably under TYPE, par. 11) that the first proposal established the familiotype for all time, and that it does not change, though its name may be changed. The proposed citation would be incorrect and confusing from a bibliographical standpoint. (See also comment under par. 26, following.) The final wording of the section should of course fit the action taken on date of publication (12-13). **O.K.**

CITATION:

26 : I recommend a proposal that authors and dates shall not be given for suprageneric names, in the way that they are customarily given for generic and trivial names, unless the person referring to the names needs to distinguish between them, as in discussing the suprageneric nomenclature of a group. I recommend this as a step toward minimising the personal element as much as possible. Perhaps "X— Y—" will not propose so many tribes, sections, subsections, etc., if he knows that the name "X— Y—" will not appear every time one is cited.

Some remarks under Author (25) are pertinent here: For substitute names, I should cite them (using Mr. Hemming's example) as "RIODINIDAE Grote, 1895," or "RIODINIDAE Grote, 1895 (ERYCINIDAE) Swainson," or "RIODINIDAE Grote, 1895 (pro ERYCINIDAE Swainson, 1827) (=X-IDAE Westwood, 1840)," depending on the detail which is necessary or desired. **No. I think authors and dates should be cited.**

CHANGE OF NAME OF FAMILIOTYPE:

14 : Approve, except for the back-dating. I recommend opposing that as discussed above under 25 and in part under 26. Back-dating is a method that would indeed ensure what I have called the "continuity of the type concept," i.e. that a family name should follow the nomenclatorial course of its original familiotype. I oppose back-dating partly because it is bibliographically incorrect and misleading, and partly because the rules would be simplified that much more to eliminate it here and elsewhere. The goal of continuity, in which I concur with Mr. Hemming, can be attained more simply and directly by writing the provision into the section on Type (par. 11), q.v. **O.K.**

29 : Approve. **O.K.**

15 : Approve, but I should make it part of the general problem of synonymy as I propose to treat it. (See next section, 21-24, under (xx).) **O.K.**

21-24 : I should like first to dispose of my item(x) which is discussed in paragraph 22 (" Situation arising when a family name is not based upon the name of a genus included in the family ") : I doubt that this situation exists today, and I recommend that the section be omitted. **O.K. Omit.**

It seems probable that this situation has been incorrectly raised to the status of a current problem by *Opinion* 141, Summary (3), Note, which refers to " a few *well-established* family names proposed by early authors " but not based on included genera (italics mine, here and below). Actually, however, in *Opinion* 133 (upon which 141 is based) Dr. Stiles appears only to have been mentioning the existence of such names in early times, without meaning to carry the problem to the present (*Opin.* 133, p. 42, last paragraph : " Since (with the exception of isolated instances by *early authors*) family names are based . . ."). Example, in Diptera : The family of the small-headed flies was first called INFLATA and then VESICULOSA (VESICULEUX), both names referring to the swollen appearance of the body, and not based on generic names. But these names were soon abandoned in favour of such names as ACROCERIDAE, HENOPIDAE and CYRTIDAE, based respectively on *Acrocera*, *Henops* and *Cyrtus*.

21-24, my item (xx) : I recommend opposing Mr. Hemming's view, and instead supporting a proposal that family names should *not* be changed when the familiotype is found to be a junior synonym, whether objective or subjective. Mr. Hemming has directed the discussion to the " exceptional " feature, but advocates of the proposal in (xx) above believe that it should be a regular procedure to aid in future stability of family names, as Mr. Hemming has agreed in part in paragraph 15. I see no great gain or value in being able to recognise the current name of the familiotype in the family name. Non-systematists have neither need nor care for that, and systematists have no need, for any systematist worthy of his salt in a group will necessarily know both the senior and junior synonyms. One could make an equally strong case that it would be an " extremely great gain " to be able to recognise from the family name what is the original familiotype. **Agree with Sabrosky.**

Numerous examples can be cited where such a rule would have avoided changes of names and thus contributed to stability. The sixteen families of Diptera that involve Meigen 1800 vs. 1803 names for their familiotypes are good examples : *Chironomus* 1803 could change to *Tendipes* 1800 and the family name could have remained CHIRONOMIDAE. French writers who have used LARVAEVORIDAE (*Larvaevora* Meigen 1800) still call the flies " les tachinides." One family of Diptera has had four names as a result of synonymy (successively, RHYPHIDAE, ANISOPODIDAE, PHRYNEIDAE, SYLVICOLIDAE), when one name RHYPHIDAE could have sufficed. With all the changes that are bound to come when all five or ten million species of insects are known, let us plan to avoid any more changes than absolutely necessary.

FAMILY NAMES IN COMPETITION :

16-19 : I agree with Mr. Hemming in wishing to base a decision on priority of some sort, but I recommend that our Committee support the priority of the oldest proposed family name, and not of the designation of the genotype. His reason, as given above (" to ensure," etc.), will apply equally well to my method : Determine the oldest proposed family name, in which will appear the name of the familiotype, and the concept of that family will follow the type species of that genus. It seems to me that this is more simple and direct, and would also tend to preserve continuity and usage in the older family names. Suppose for example that a family proposed in 1820 was based on a large genus whose type was not fixed until 1880. In 1920 another family was proposed for an aberrant genus known from a single species since 1810. In 1930 an author who combined the two families would be obliged under Mr. Hemming's proposal to reject the century-old family name and adopt the younger one, which happened to have the oldest fixed genotype. The principle of oldest proposed family name also enters into paragraphs 20 and 27-28. **Agree with Sabrosky.**

20 : I recommend omitting this as a separate provision (see my second paragraph of discussion). **O.K.**

27-28 : Approve, but the rule needs to say more than merely that the junior homonym is to be rejected. What shall take its place ? Specialists can, of course, apply as they did for *Opinion* 140, but will they always have to ? Are there any directions that can be given to apply to most cases ? On the other hand, perhaps homonymy in family names is so relatively uncommon that all cases should be arbitrarily decided by the Commission upon advice from specialists in the affected group. It will not always be as easy to arrive at distinct family names as it was in the case of MEROPIDAE and MEROPEIDAE. Incidentally, it occurs to me now that MEROPSIDAE would have been a better name for the former, by carrying the full generic name and thus being the more characteristically distinct from MEROPEIDAE. **O.K.**

(*b*) NAMES OF UNITS BETWEEN SUBFAMILY AND GENUS

34-38 : Approve, except for the saving clause on the supergeneric names. The argument of " preventing displacement of an established " name by downgrading of an older name from a higher level could as well be applied to any category. Treating them thus would set up a special case within a case. If they are to be based on generic names, by adding some appropriate ending to the stem of the supergenotype, it would seem to me best to incorporate them completely into the system. **O.K. but delete " Supergenus."**

(*c*) NAMES OF SUPERFAMILIES

39-42 : Approve, except for the saving clause on the priority *inter se* of the names. It seems to me that these also may as well be co-ordinated completely with the system. Their existence should never complicate the priority of family names, for they will be formed from already existing family names. If superfamilies are combined, of course, then one may reasonably require, as with the union of two or more families (par. 16-19), that one should select the first to be proposed as a superfamily. **O.K.**

(*d*) OFFICIAL LIST AND OFFICIAL INDEX

43-48 : Approve, though the book work of this kind is becoming a bit deep. In a paper of mine submitted to the Commission on the subject of family names in March 1946 (not published*) ; see also my article in *Amer. Nat.* **81** : 153-160, 1947), I myself proposed an Official List of Family Names. I envisioned, however, not a maintained list to which names would be added periodically, but a planned undertaking to encompass the whole subject and produce a solid and useful reference work (see last half of the third paragraph of my discussion). **O.K.**

(*e*) ADVICE SOUGHT

A few points that should be covered in the proposal on family names, in addition to several already mentioned, are as follows :—

(x) In paragraph 14 (cases of generic homonymy), what about those cases in which the junior homonym is not renamed, but is replaced by the next available synonym ? If the junior homonym and its synonym are isogenotypic, no harm is done, for the type species is the same. If, however, they are not isogenotypic, there is a break in the continuity. (Example : ORTALIDAE (Diptera), based on *Ortalis*, preoccupied in Aves ; the name now commonly used is OTITIDAE, based on *Otites*, a different genus than *Ortalis*, though fortunately a very near relative.) **The junior homonym should be renamed and this should become the valid name !**

(xx) Misidentified genotypes and their relation to the family name problem.

(xxx) The proposed endings for superfamily and tribe are widely used in insects, but not so in other groups. Should consideration be given to setting up different rules for different Phyla, where uniformity may already have been reached on other endings (such as " -IFORMES ") ? **Optional upon appl. to Comm. "-OIDEA" and "-INI" O.K.**

*The paper here referred to is here published as Document 3/5 (pp. 171-176).

APPENDIX 2 TO DR. W. I. FOLLETT'S LETTER OF 23RD JULY 1952

DR. J. CHESTER BRADLEY'S COMMENTS ON THE SUGGESTIONS MADE IN THE PAPER BY MR. CURTIS W. SABROSKY ANNEXED TO DR. FOLLETT'S LETTER AS APPENDIX 1, ENCLOSURE 2

(For a separate statement furnished by Dr. J. Chester Bradley, see Document 3/37)

Family and Co-ordinate Names (Reference Z.N.(S.)357) : Comments on Mr. Curtis W. Sabrosky's Paper

By J. C. Bradley

Paras. **4-6**

I suggest rewording Art. 4 to read : " The name of a family is to be formed by adding the suffix " -IDAE," the name of a subfamily by adding " -INAE " to the stem of the name of its type genus, " stem " for this purpose being interpreted by the two paragraphs that follow :—

(*a*) In case of a name which is not, or the terminal element of which is not a classical noun, the " stem " within the meaning of this Article shall be that portion of the name that shall be first selected to receive the suffix " -IDAE," " -INAE," or any other patronymic (or equivalent) suffix intended to convert that generic name into the name of a group higher than genus.

(*b*) In case the name or the last element of the name consists of a classical noun, the " stem " as here used shall be understood to consist of the genitive form of that noun without its case-ending.*

So far as barbarous words or arbitrary combinations of letters are concerned, it is obvious that any result is arbitrary. A rule in a Schedule could be helpful, but only advisory. The person who first establishes a plural group name based on such a barbarous name should have the privilege of establishing what that stem is to be.

Paras. **7-10**

The rule above suggested is mandatory and can lead to but one result. Anything else is a misspelling. A correction should be required whenever such a mistake is found to exist. But, in his article on emendations, Mr. Hemming has been at pains to differentiate such a *change of incorrect spelling* from an emendation. From this point of view, a technical emendation of a family name could result only from an emendation of the generic name on which it is based.

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*This will not always be the classical form of the stem, particularly in the case of vowel stems, but it will be that part, form, or contraction of the stem that the ancients would themselves have used in combination with a suffix commencing with a vowel, is entirely appropriate for neo-Latin, and is in fact the usage of the most careful linguists among zoologists since the days of Linnaeus. Although my suggestion was written before I had read Dr. Grensted's paper (1947, *Ent. Mon. Mag.* **83** : 137–141) it is precisely the objective part of the solution that he proposed at the top of p. 141, l. c. It seems to have been the first sentence of the same paragraph that suggested the emendation of Art. 4 adopted at Paris ; but that part is subjective and not necessary, in fact may be looked upon more as an explanation of the result sought by the rule laid down in the last sentence. The rare case (as perhaps CISIDAE) not covered by the rule could be handled as an exception by the Commission.

Para. **11**

Concur with Sabrosky.

Paras. **30-33**

Concur with Sabrosky but go further. All suprageneric groups based on a generic name must be co-ordinate, even though called (as many old authors have done) by some unrecognised name, such as legion.

Paras. **12-13**

Strongly concur with Sabrosky. Am neutral on, but willing to accept, French vernaculars if there is a general feeling that they should be accepted.

Para. **25**

Concur with Sabrosky, but with the understanding that the author to be cited is the author of the suprageneric name, regardless of rank or termination assigned it. I should add

> " If it is desired to cite the author who first assigned a particular rank and limit to a suprageneric group, this may be done by adding in parentheses ' (sense of so-and-so, such-and-such a data).' "

Paras. **21-24**

Sabrosky's item (X). Concur with Sabrosky.

Sabrosky's item (XX). There are three classes of cases.

(*a*) The name of the type genus is a junior homonym.

(*b*) The name of the type genus is an objective junior synonym.

(*c*) The name of the type genus is subjectively considered to be a junior synonym.

Once the type genus has been established, it would be contrary to all principles of types to change it, and therefor not allowable.

In cases (*a*) and (*b*) there is no change in the type genus involved, it is only a question of the name under which it is known.

In case (*c*) it is only a matter of opinion that the senior name applies to the same group. We are confronted with two *nominal* genera, and to shift from one to the other would be a change of type, therefore not theoretically allowable.

In such a case it is clear that Sabrosky's plan is correct ; the junior synonym remains the name of the nominal type genus and must (by definition) be that upon which the valid family name is based, even by authors who subjectively reject it for the genus.

In case (*b*) it is theoretically possible to base the family name upon the name of either objective synonym, for they are but two names for the same type genus. I have somewhat reluctantly, in the interest of continuity, come around to Sabrosky's view here also. Nevertheless I strongly believe that in such cases the better solution is for the Commission by plenary action to

conserve the junior synonym at the generic name level. I should favour such a rule as

> " In case the name of the type genus of a suprageneric group is found to be a junior objective synonym, the Commission shall, under its plenary power, conserve that name, unless there are strong reasons for doing otherwise."

In case (*a*) we cannot use the junior homonym, because of conflict or possible conflict with suprageneric names based on the senior homonym. There should be a rule directing the Commission to suppress the senior homonym if it is without possible utility in its Phylum. If this cannot be done, and there is no objective synonym of the junior homonym, then there remains only one legal recourse. This is to deliberately create an objective synonym to replace the junior homonym, which then becomes available (*a*) for basis of the family name, and (*b*) as a substitute for the junior homonym that is the unavailable name of the type genus, for any one who rejects all subjective synonyms, and then to use this objective synonym as basis for the family name. If the result is objectionable, the Commission can suspend the rule in favour of another name. It is not possible in any other way to select a new family name based on some other nominal genus (as OTITIDAE instead of ORTALIDAE) since to do so would be to change the type genus, which is illegal. This could not be done even if the replacing name were a subjective synonym of the junior homonym.

Paras. **27-28**

Agree with Sabrosky, but the case is a little worse than he points out. Hemming's suggestion " to be rejected as a junior homonym " produces an absolute impasse. The type-genus of the suprageneric name is that which gives its stem to the homonymous suprageneric name ; but the genus is not a homonym and its name is valid. To shift the type genus is impossible under the rules, and would nullify the existence of any type. Yet there is only one way to form a suprageneric name under the *Règles* from that generic name. The only escape is by suspension of the rules and plenary action.

The principle is the same throughout all suprageneric categories, and since there are many cases of two or more genera with identical stems, I shudder to think of the number of homonymous names that may exist at tribal level or above that have never been noted.

Paras. **34-36**

Agreed to.

Paras. **37-38**

Supergenus. The name of a supergenus should be the name of the nomino-typical genus and subgenus without changed ending. In citation it should be distinguished from genus only by insertion of the word " Supergenus " before it, but would not be included in the full specific name of an included species. Such is my impression, but I may be wrong. Can any one cite instances where supergenus is employed with a special termination ?

Paras. **39-42**

I support Sabrosky's suggestion, but, as in the case of family name, it is the first given suprageneric name that is the nominotypical genus of tribe, subfamily, family and superfamily (not, at each level, the name first to be accorded that particular rank).

We speak of the type-genus of tribe, of subfamily and of family. We should do the same for superfamily. We do not speak of type-genus of tribe, type-tribe of subfamily, type-subfamily of family, nor should we speak of type-family of a superfamily. It is, however, perfectly permissible for descriptive purposes to speak of the typical or nominotypical family, subfamily or tribe.

Paras. **43-48**

I agree to Hemming's proposals, with three suggestions :—

(a) that each suprageneric name be entered in the form in which it was first established, with author and date, followed by its first usage, if any, with each of the authorised suffixes -ini, -inae, -idae, and -oidea.

(b) that the list be entitled *Official List of Suprageneric Names in Zoology* ;

(c) that somewhere a suprageneric name be defined as any name of a group higher than genus based on the stem (or its equivalent) of a contained genus with a suffix added to indicate its rank.

Point **(X)**

See my discussion above under heading 21-24, case (a).

Point **(XX)**

The most serious difficulty in family names arises in the case of a name the type-genus of which actually belongs to a taxonomic group far removed from that to which that name was believed to belong. All such cases should be brought to the attention of the Commission as rapidly as possible. A regulation should instruct the Commission under its plenary power to set aside the type-species of such genera, and to establish new type-species that will conserve the accepted meanings of the generic names.

I think the above covers what Dr. Sabrosky has in mind. However, it is not quite what he says. "Misidentified genotypes" actually means type-species of a genus which is not the species that the describer of the genus or the selector of the type-species actually meant when the species was designated type. Such cases come up in any case for review by the Commission, which can be relied upon (I hope) not to decide them in such a way as to upset a family name.

Point **(XXX)**

I have no objection to other terminations for tribe and superfamily being established for phyla where very strong demand exists among specialists. I hope it will not be necessary and that specialists will change over. The termination " -ACEA " has been quite universally employed in Mollusca for superfamily, but recently some authors (or at least one) have been changing over to " -OIDEA."

APPENDIX 3 TO DR. W. I. FOLLETT'S LETTER OF 23RD JULY 1952

OBSERVATIONS BY MR. CURTIS W. SABROSKY ON THE PROPOSALS IN HIS PAPER RELATING TO FAMILY NAMES ANNEXED TO DR. FOLLETT'S LETTER AS APPENDIX 1, ENCLOSURE 2, MADE BY DR. J. CHESTER BRADLEY IN THE PAPER ANNEXED TO THE FOREGOING LETTER AS APPENDIX 2.

Comments on Dr. Bradley's Comments

By Curtis W. Sabrosky

4-6. The suggested rewording of Article 4 seems to cover the matter, though I am not sure that it agrees with my views (which I still hold on the stem of nouns like *Acrocera* and *Leptocera*). However, perhaps I have misinterpreted the comments.

30-33. I agree. The "going further" that Dr. Bradley suggests was hinted at in my sections (b) and (c), but, if all are to be co-ordinate, this is indeed the appropriate place to say so.

25. I agree. The "understanding" should indeed be specified, if the final decision on 12-13 follows our line of reasoning.

21-24, my item (XX). I am glad to note that Dr. Bradley agrees with my view, albeit reluctantly with respect to his case (b). However, I do not agree that it would be better to have the Commission do it by plenary action. The best hope for the future in the mass of names and problems that will come with another few million species is to refer *fewer* things to the Commission, not more. Under case (a) I am interested to see that Dr. Bradley expresses a view that I have often maintained: that in cases of homonymy we should "deliberately create an objective synonym to replace the junior homonym." in order to avoid changing the type. I suppose the Commission could suppress the senior homonym in cases where the latter is of no consequence in its group (e.g., a junior synonym), but this again would make us dependent upon the Commission. Can we not devise Rules that taxonomists can work in the great bulk of cases, and leave the Commission free to resolve difficulties when (or if) they arise?

27-28. It is perhaps worse than I pointed out, but I hope not as bad as Dr. Bradley fears. Whether such cases are common or uncommon, the only solution does seem to be arbitrary action under the plenary powers, with the advice of specialists concerned.

37-38. I would enthusiastically accept Dr. Bradley's suggestion for treating supergenus without employing any characteristic termination, and accordingly (I presume he meant) without making it co-ordinate with other suprageneric names.

(XX). There is a difference in wording but not in essentials. I have no objection to the statement in the first paragraph. But, of course, in the final analysis, the interpretation of the type genus of a family can only rest on the identity of the type species of the genus. If the latter has been erroneously interpreted (="misidentified genotypes" of my brief reference), the discovery of the fact causes the "most serious difficulty in family names" to arise.

(XXX). Uniformity would indeed be desirable. Probably I should not have introduced the question, but left it for workers in other phyla where practices have become stabilised in other endings (if there are any).

APPENDIX 4 TO DR. W. I. FOLLETT'S LETTER OF 23RD JULY 1952

COMMENTS RECEIVED BY DR. FOLLETT FROM MEMBERS OF THE NOMENCLATURE COMMITTEE OF THE SOCIETY OF SYSTEMATIC ZOOLOGY, OTHER THAN FROM MR. CURTIS W. SABROSKY, THE COMMENTS RECEIVED FROM WHOM WERE ANNEXED TO DR. FOLLETT'S LETTER AS APPENDIX 1, ENCLOSURE 2, AND APPENDIX 3

Enclosure 1 to Appendix 4 to Dr. Follett's letter

Communication furnished by Dr. W. I. FOLLETT

I adopt the discussion and recommendations by Mr. Curtis W. Sabrosky on the subject of names of families and co-ordinate categories, and his comments upon Dr. Bradley's remarks, as setting forth my views more accurately than I could have expressed them.

Enclosure 2 to Appendix 4 to Dr. Follett's letter

Communication furnished by Professor CHARLES D. MICHENER

I agree with Bradley's comments on Sabrosky's paper.

Enclosure 3 to Appendix 4 to Dr. Follett's letter

Communication furnished by Professor HENRY TOWNES

(*Letter dated 20th May* 1952)

I read over the material with interest and express the following opinions on the questions of family and subfamily names :—

(2) The amount of discussion and the complications regarding family names indicate confusion of ideas and a rather unworkable system. I believe that family (also subfamily, tribe, and superfamily) names should be based on the stem of the oldest available generic name included. This method avoids all arguments about date of publication, author, homonyms, synonyms, etc. It also is a method that is workable with available bibliographic tools and results in names at least as much in agreement with current usage as any other. If family names were ever subject to rules, probably the Stricklandian code had as much influence as any, as it was in force at the time most family names were coming into use. That code, I am told, recommended use of the name of the oldest included

genus as the stem of the family name. Why are we rejecting it now ? Probably because so much of the talking and writing about family names has been done by muddlers, and the basic ideas have been lost in the furor about " familiotypes."

The only place I have found difficulty in using the valid name of the oldest included genus as the basis of the family name is where there are two or more genera of the same age. Here, it seems best to use the principle of first reviser, selecting the one first used as the basis of a supergeneric group name.

Enclosure 4 to Appendix 4 to Dr. Follett's letter

Communication furnished by Professor JOHN W. WELLS

(*Letter dated 11th June* 1952)

NAMES OF FAMILIES AND CO-ORDINATE CATEGORIES

(*Summary, Comment and Recommendations by Sabrosky*) :

Paras.	**4-6.**	Approve as recommended by Sabrosky.
,,	**8-9.**	Omit ,, ,, ,, ,,
Para.	**10.**	Approve ,, ,, ,, ,,
,,	**11.**	Approve ,, ,, ,,
Paras.	**12-13.**	Approve as recommended by Hemming, not Sabrosky, except that no exceptions of any sort should be allowed.
,,	**30-33.**	Approve.
Para.	**25.**	Disapprove the saving clause. The author of a family, and date, can only be he who first used termination " idae," just as in case of substitute generic names.
,,	**26.**	Approve as recommended by Hemming, not Sabrosky.
,,	**14.**	,, ,, ,, ,, Sabrosky.
,,	**29.**	,, ,, ,, ,, ,,
,,	**15.**	Disapprove as recommended by Hemming or Sabrosky. Family names should be treated exactly as generic names.
Paras.	**27-28, 34-38, 39-42.**	Approve as recommended by Sabrosky.
,,	**43-48.**	Disapprove strongly of any kind of *Official List* or *Official Index*, on grounds of too much work for Commission, involves perpetuation of errors—a most unscientific principle, and just plain lack of any sound reason for such a thing.

Enclosure 5 to Appendix 4 to Dr. W. I. Follett's letter

Communication furnished by Dr. JOHN T. ZIMMER

Family names should uniformly end in " -IDAE " ; subfamily names, in " -INAE." Names to date from the first use of the name, regardless of its ending, in a true suprageneric sense. Family names should not be changed when the name of the type genus is changed (=repeal of Article 5) which may require placement of the name of the type genus on the *Official List*. In case of homonymy, the Commission may change a family name.

Value of citing author and date of family names is questionable. In any case priority is *not* to be on the basis of the family name whose type genus first had its type species elevated to that position ! Should be on strict priority of the names themselves or the appropriate stems with, perhaps, an ending other than " idae."

Tribal names may end in " -INI " but Supergenera are hardly entitled to consideration other than as conveniences, not true nomenclatural groups. They should not be dignified by regular endings.

Superfamily names co-ordinate with family names, to end in " oidea."

An *Official List of Family Names* seems desirable ; also an *Official Index of Rejected and Invalid Names.*

DOCUMENT 3/35

Statement of the views of the
AMERICAN COMMITTEE ON ENTOMOLOGICAL NOMENCLATURE

Enclosure to a letter, dated 24th July 1952, from
Professor CHARLES D. MICHENER, Chairman

Family Names (Commission's Reference Z.N.(S.)357)

In presenting the views of the American Committee on Entomological Nomenclature, it has been found convenient to refer to the paragraphs of Mr. Hemming's proposal (1952, *Bulletin of Zoological Nomenclature*, **7** : 61-94). The paragraphs below are therefore numbered to correspond to the paragraphs of Mr. Hemming's article. One general simplification which is recommended is that instead of repeatedly stating that specialists can apply to the Commission if they wish exceptions to the rules made, a final statement to this effect would suffice.

(a) Names of Families and Subfamilies

STEM :

Paras. **4-6.**

We recommend rewording Art. 4 to read : " The name of a family is to be formed by adding the suffix " -IDAE," the name of a subfamily by adding " -INAE " to the stem of the name of its type-genus, " stem " for this purpose being interpreted by the two paragraphs that follow :—

(a) In case of a name which is not, or the terminal element of which is not a classical noun, the " stem " within the meaning of this article shall be that portion of the name that shall be first selected to receive the suffix " -IDAE," " -INAE," or any other patronymic (or equivalent) suffix intended to convert that generic name into the name of a group higher than genus.

(b) In case the name or the last element of the name consists of a classical noun, the " stem " as here used shall be understood to consist of the genitive form of that noun without its case-ending.*

So far as barbarous words or arbitrary combinations of letters are concerned, it is obvious that any result is arbitrary. A rule in a Schedule could be helpful, but should be only advisory. The person who first establishes a plural group name based on such a barbarous name should have the privilege of establishing what that stem is to be.

*This will not always be the classical form of the stem, particularly in the case of vowel stems, but it will be that part, form, or contraction of the stem that the ancients would themselves have used in combination with a suffix commencing with a vowel, is entirely appropriate for neo-Latin, and is in fact the usage of the most careful linguists among zoologists since the days of Linnaeus.

Paras. **7-10**

We recommend that these sections be omitted, as a contribution toward making the rules more simple and direct. Any changes that would be required as a result of general enforcement of the rules on stems will be of such minor nature as to be unimportant. The above recommended rule on stems is mandatory and can lead to but one result. Anything else is a misspelling. A correction should be required whenever such a mistake is found to exist. But, in his article on emendations, Mr. Hemming has been at pains to differentiate such a *change of incorrect spelling* from an emendation. From this point of view a technical emendation of a family name could result only from an emendation of the generic name on which it is based.

TYPE

Para. **11**

Approve, but suggest that it would be appropriate to add here the provision, which Mr. Hemming has elsewhere, that the originally established *familiotype* is not subject to change, though its *name* may sometimes have to be changed. (Cf. discussion of paragraphs 25 and 14.)

SUBFAMILY NAMES

Paras. **30-33**

We approve but would go farther and state that all suprageneric groups based on a generic name must be co-ordinate, even though called (as many old authors have done) by some unrecognised name, such as legion.

DATE OF PUBLICATION

Paras. **12-13**

We recommend a rule that a family name dates from its first publication as a suprageneric group name based on an included genus, regardless of the ending employed and regardless of what the author called his suprageneric category (family, stirps, etc.). This rule will avoid attempting to decide on a date before which some names may be validated on other than " -IDAE " endings (1850 suggested by Mr. Hemming), and will avoid the need for and the trouble with applications to save those old names. Actually, many of the old names which used other endings, before the " -IDAE " ending was widely (and quickly) accepted, are the old familiar families which everyone will wish to preserve anyway ; e.g. TIPULARIAE Latreille, CULICIDES Billberg, TABANII Latreille, etc. Example of the difficulties that may arise by insistence on the " -IDAE " form : The family name of the moth flies dates from PSYCHODITES Newman 1834, or from PSYCHODIDAE Bigot 1854. But if one dates that only from the " -IDAE " ending in 1854, then the oldest name for the family is actually FLEBOTOMIDAE Rondani 1840 (emended PHLEBOTOMIDAE Walker 1851).

Mr. Hemming mentions vernacular names, especially as used by French authors. We see no reason why these might not be accepted for the older names (e.g. before 1850), provided, however, that it is clear that they are *group* names (suprageneric) and not merely a way of referring in the plural to the species of one genus.

AUTHOR

Para. **25**

Approved, with the understanding that the author to be cited is the author of the suprageneric name, regardless of rank or termination assigned it. We should add, " If it is desired to cite the author who first assigned a particular rank and limit to a suprageneric group, this may be done by adding in parenthesis ' (sense of so-and-so, such-and-such a date).' "

Also, we see no advantage to be gained by citing a substitute name by the author and date of the name which is replaced, if the Rules somewhere make clear (probably under TYPE, par. 11) that the first proposal establishes the familiotype for all time, and that it does not change, though its name may be changed. The proposed citation would be incorrect and confusing from a bibliographical standpoint. (See also comment under para. 26, following.) The final wording of the section should of course fit the action taken on date of publication (12-13).

CITATION

Para. **26**

We recommend that authors and dates shall not be given for suprageneric names, in the way that they are customarily given for generic and trivial names, unless the person referring to the names needs to distinguish between them, as in discussing the suprageneric nomenclature of a group. We recommend this as a step toward minimising the personal element as much as possible. Perhaps a person will not propose so many tribes, sections, subsections, etc., if he knows that this name will not appear every time one is cited.

Some remarks under Author (25) are pertinent here. For substitute names, we should cite them (using Mr. Hemming's example) as " RIODINIDAE Grote, 1895," or " RIODINIDAE Grote, 1895 (ERYCINIDAE Swainson)," or " RIODINIDAE Grote, 1895 (pro ERYCINIDAE Swainson, 1827) (=X-IDAE Westwood, 1840,") depending on the detail which is necessary or desired.

CHANGE OF NAME OF FAMILIOTYPE

Para. **14**

Approve, except for the back-dating (see our comments on paragraph 25). Back-dating is a method that would ensure the continuity of the type concept, i.e. that a family name should follow the nomenclatorial course of its original familiotype. We oppose back-dating partly because it is bibliographically incorrect and misleading, and partly because the rules would be simplified that much more to eliminate it here and elsewhere. The goal of continuity, in which we concur with Mr. Hemming, can be attained more simply and directly by writing the provision into the section on Type (par. 11), q.v.

Para. **29**

Approved.

Para. **15**

Approved, but we should make it part of the general problem of synonymy (see notes on paragraphs 21-24, *b*).

Paras. **21-24**

We feel that there are two problems here. One (paragraph 22) has to do with the "situation arising when a family name is not based upon the name of a genus included in the family." We doubt that this situation exists today, and recommend that the section be omitted. It seems probable that this situation has been incorrectly raised to the status of a current problem by *Opinion* 141, Summary (3), Note, which refers to "a few *well-established* family names proposed by early authors" but not based on included genera (italics ours, here and below). Actually, however, in *Opinion* 133 (upon which 141 is based) Dr. Stiles appears only to have been mentioning the existence of such names in early time, without meaning to carry the problem to the present (*Opinion* 133, p. 42, last paragraph: "Since with the exception of isolated instances by *early authors*) family names are based . . ."). Example, in Diptera: The family of the small-headed flies was first called INFLATA and then VESICULOSA (VESICULEUX), both names referring to the swollen appearance of the body, and not based on generic names. But these names were soon abandoned in favor of such names as ACROCERIDAE, HENOPIDAE and CYRTIDAE, based respectively on *Acrocera*, *Henops* and *Cyrtus*.

The second problem has to do with the proposal that family names should not be changed when the familiotype is found to be a junior synonym. We recommend that family names should *not* be changed when the familiotype is found to be a junior synonym, whether objective or subjective. Mr. Hemming has directed the discussion to the "exceptional" feature, but we believe that this should be a regular procedure to aid in future stability of family names, as Mr. Hemming has agreed in part in paragraph 15. We see no great gain or value in being able to recognise the current name of the familiotype in the family name. Non-systematists have neither need nor care for that, and systematists have no need, for any systematist worthy in a group will necessarily know both the senior and junior synonyms. One could make an equally strong case that it would be an "extremely great gain" to be able to recognise from the family name what is the original familiotype.

Numerous examples can be cited where such a rule would have avoided changes of names and thus contributed to stability. The sixteen families of Diptera that involve Meigen 1800 vs. 1803 names for their familiotypes are good examples: *Chironomus* 1803 could change to *Tendipes* 1800 and the family name could have remained CHIRONOMIDAE. French writers who have used LARVAEVORIDAE (*Larvaevoira* Meigen 1800) still call the flies "les tachinides." One family of Diptera has had four names as a result of synonymy (successively, RHYPHIDAE, ANISOPODIDAE, PHRYNEIDAE, SYLVICOLIDAE), when one name (RHYPHIDAE) could have sufficed. With all the changes that are bound to come when all five or ten million species of insects are known, let us plan to avoid any more changes than absolutely necessary.

A more detailed discussion of the matter follows: There are three classes of cases in which the name of the familiotype may change.

(*a*) The name of the type genus is a junior homonym.

(*b*) The name of the type genus is an objective junior synonym.

(*c*) The name of the type genus is subjectively considered to be a junior synonym.

Once the type genus has been established, it would be contrary to all principles of types to change it, and therefore not allowable.

In cases (*a*) and (*b*) there is no change in the type genus involved, it is only a question of the name under which it is known.

In case (*c*) it is only a matter of opinion that the senior name applies to the same group. We are confronted with two *nominal* genera, and to shift from one to the other would be a change of type, therefore not theoretically allowable.

In such a case it is clear that the above recommended plan is correct; the junior synonym remains the name of the nominal type genus and must (by definition) be that upon which the valid family name is based, even by authors who subjectively reject it for the genus.

In case (*b*) it is theoretically possible to base the family name upon the name of either objective synonym, for they are but two names for the same type genus. Another alternative in such cases is for the Commission by plenary action to conserve the junior synonym at the generic name level. We might have such a rule as :—

> " In case the name of the type genus of a suprageneric group is found to be a junior objective synonym, the Commission shall, under its plenary power, conserve that name, unless there are strong reasons for doing otherwise."

In case (*a*) we cannot use the junior homonym, because of conflict or possible conflict with suprageneric names based on the senior homonym. There should be a rule directing the Commission to suppress the senior homonym if it is without possible utility in its phylum. If this cannot be done, and there is no objective synonym of the junior homonym, then there remains only one legal recourse. This is to deliberately create an objective synonym to replace the junior homonym, which then becomes available (*a*) for basis of the family name, and (*b*) as a substitute for the junior homonym that is the unavailable name of the type genus, for any one who rejects all subjective synonyms, and then to use this objective synonym as basis for the family name. If the result is objectionable, the Commission can suspend the rule in favour of another name. It is not possible in any other way to select a new family name based on some other nominal genus (as OTITIDAE instead of ORTALIDAE) since to do so would be to change the type genus, which is illegal. This could not be done even if the replacing name were a subjective synonym of the junior homonym.

FAMILY NAMES IN COMPETITION

Paras. **16-19**

We agree with Mr. Hemming in wishing to base a decision on priority of some sort, but recommend that it be the priority of the oldest proposed family name, and not of the designation of the genotype. His reason (" to ensure," etc.), will apply equally well to our method : Determine the oldest proposed family name, in which will appear the name of the familiotype, and the concept

of that family will follow the type species of that genus. It seems to us that this is more simple and direct, and would also tend to preserve continuity and usage in the older family names. Suppose for example that a family proposed in 1820 was based on a large genus whose type was not fixed until 1880. In 1920 another family was proposed for an aberrant genus known from a single species since 1810. In 1930 an author who combined the two families would be obliged under Mr. Hemming's proposal to reject the century-old family name and adopt the younger one, which happened to have the oldest fixed genotype. The principle of oldest proposed family name also enters into paragraphs 20 and 27-28.

Para. **20**

We recommend omitting this as a separate provision, as it would be covered by a general statement (see first paragraph of this discussion of family names).

Paras. **27-28**

Mr. Hemming's suggestion " to be rejected as a junior homonym " produces an impasse. The type genus of the suprageneric name is that which gives its stem to the homonymous suprageneric name ; but the genus is not a homonym and its name is valid. To shift the type genus is impossible under the rules, and would nullify the existence of any type. Yet there is only one way to form a suprageneric name under the *Règles* from that generic name. The only escape is by suspension of the rules and plenary action.

(b) Names of Units between Subfamily and Genus

Paras. **34-36**

Approved.

Paras. **37-38**

Approve, except for the saving clause on the suprageneric names. The argument of " preventing displacement of an established " name by down-grading of an older name from a higher level could as well be applied to any category. Treating them thus would set up a special case within a case. If they are to be based on generic names, by adding some appropriate ending to the stem of the supergenotype, it would seem to me best to incorporate them completely into the system.

We believe, however, that the name of a supergenus should be the name of the nominotypical genus, without changed ending. In citation it would be distinguished by insertion of the word " supergenus " before it.

(c) Names of Superfamilies

Paras. **39-42**

Approve, except for the saving clause on the priority *inter se* of the names. It seems that these may as well be co-ordinated completely with the system. Their existence should never complicate the priority of family names, for they will be formed from already existing family names. If superfamilies are combined, of course, then one may reasonably require, as with the union of two or more families (para. 16-19), that one should select the first to be proposed as a superfamily.

We speak of the type genus of tribe, of subfamily and of family. We should do the same for superfamily. We do not speak of type genus of tribe, type-tribe of subfamily, type-subfamily of family, nor should we speak of type-family of a superfamily. It is, however, perfectly permissible for descriptive purposes to speak of the typical or nominotypical family, subfamily or tribe.

(d) Official List and Official Index

Paras. **43-48**

We approve of Mr. Hemming's proposals, with these suggestions :—

(*a*) that each suprageneric name be entered in the form in which it was first established, with author, date, followed by its first usage, if any, with each of the authorised suffixes " -INI," " -INAE," " -IDAE," and " -OIDEA."

(*b*) that the list be entitled *Official List of Suprageneric Names in Zoology*.

(*c*) that somewhere a suprageneric name be defined as any name of a group higher than genus based on the stem (or its equivalent) of a contained genus with a suffix added to indicate its rank.

(e) Additional Problems

The most serious difficulty in family names arises in the case of a name the type genus of which actually belongs to a taxonomic group far removed from that to which that name was believed to belong. All such cases should be brought to the attention of the Commission as rapidly as possible. A regulation should instruct the Commission under its plenary power to set aside the type-species of such genera, and to establish new type-species that will conserve the accepted meanings of the generic names.

American Committee on Entomological Nomenclature.

CHARLES D. MICHENER, *Secretary*.

DOCUMENT 3/36

Statement of the views of the ENTOMOLOGISCHE GESELLSCHAFT, BASEL

Extract from a letter, dated 30th July 1952, from M. HENRY BEURET

(Note :—The numbers cited in the following statement are the numbers allotted to the paragraphs in the paper on the subject of family names by the Secretary to the International Commission on Zoological Nomenclature published in March 1952 in Part 3 of Volume 7 of the *Bulletin of Zoological Nomenclature*.)

Vol. 7. Part 3

No. **1-10** Rien de special à remarquer ; sommes d'accord avec vos propositions.

No. **11** Le problème exposé sous ce numero à été longuement discuté ; finalement nous avons décidé d'accepter vos propositions.

No. **12-14** Rien de spécial à remarquer.

No. **15** Le changement d'un nom de famille ne devrait, á notre avis, *pas être exclu d'emblée :* il nous parait necessaire de laisser une porte ouverte. Nous votons donc pour l'alternative (2).

No. **16-22** Rien de spécial à remarquer.

No. **23-24** D'accord avec vos propositions.

No. **25-30** Rien à remarquer.

No. **31** Le remplacement de la syllabe " -INAE " par " -ITAE " causerait sans doute des perturbations si graves que l'en devrait, à notre avis, *conserver* " -INAE " comme vous le proposez.

No. **32** Tout à fait d'accord avec vous.

No. **33-36** Rien à objecter.

No. **37-38** Nous ne veyons pas la *nécessité* d'introduire la catégorie de " supergenus." Néanmoins, nous ne voulons pas voter centre si d'autres systématiciens la réclame.

No. **39-40** D'accord.

No. **41-48** Rien à objecter.

DOCUMENT 3/37

By J. CHESTER BRADLEY
(*Cornell University, Ithaca, N.Y., U.S.A.*)

Statement received on 30th July 1952

Family Names

(Commission Reference Z.N.(S.)357 in *Bull. zool. Nomencl.*, Vol. 7)

Para. **4**

It is entirely clear that the unfortunate use of the French word " radical " (=English : " root ") in place of " thème " (=English " stem ") was an unintentional misuse of terms that could not have been made by a person familiar with the terminology of grammarians. Any serious attempt to literally apply it would have been disastrous : Whole families of words have the same root. The stem of stabilitas is stabilitat—and a family name based on the stem of a generic name Stabilitas would be Stabilitatidae. The root of stabilitas, in common with that of the whole group of words to which it belongs is " stá." But this root also has a weak form " sta." If the family ending were added to the root there would result a proper contraction from STA-IDAE. But the same family name would result from all nouns in the same great word group. The practical results would be so ridiculous that it is obvious that the framers' of the Berlin *Règles* in writing " radical " really meant " thème " ; the Graz Congress merely corrected the wording but did not change the intent of the original framers.

I do not agree that it is ordinarily a difficult matter to find the stem of a definite classical substantive. The lexicons show, just for this purpose, the form of the genitive as well as the nominative and all one has to do is to drop the case ending from the former and the stem remains. The only difficulty comes in certain vowel stems, and these could probably be covered with the guidance of a few simple rules in a Schedule. The real difficulty lies in determining the stem when there is no clear indication as to what classical word was used to form the terminal element of the generic name, or in cases where the generic name is avowedly based on a barbaric word, or is an arbitrary collection of letters. In all these cases I believe that the author first using the genus as the type of a group name of rank higher than genus, and therefore forced to select a stem, or a combination of letters that is the equivalent of a Latin stem for the barbaric or other word, in order to add a patronymic or equivalent ending, should be allowed the privilege of selecting such part of the generic name to be the stem as he sees fit, and that the regulations should provide that once such a selection has been made, the part so selected shall become the stem of that word, in the sense meant by the rule for forming family and other similar names.

I quite agree that the solution adopted at Paris is inadequate and must be revised. Further discussion will be from the viewpoint of Article 4 of the *Règles* before it was amended.

Para. **6**

The addition of a schedule giving help in finding the actual stem of a classical word, and giving suggestions as to how to proceed to select letters that should serve as the equivalent of a stem in the case of a non-classical stem would be desirable. It should contain instructions as to how to use a classical lexicon for the former purpose.

Para. **8**

Is it not incorrect to speak of the emendation of a family name in the sense that emendation is applied to generic names ?

The directions of Article 4 are definite : Start with a generic name, find its stem, add the ending " -IDAE " or " -INAE." It can only come out one way,* if it does not the rule has not been followed, and the family name is misspelled. But the rule is mandatory ; therefore a change of the incorrect spelling—not an emendation—is required.

Suppose we were to reword Article 4 as follows :

The name of a family is to be formed by adding the suffix " -IDAE, " the name of a subfamily by adding " -INAE " to the stem of the name of its type-genus, " stem " for this purpose being interpreted by the two paragraphs that follow :

(*a*) In case of a name which is not, or the terminal element of which is not a definite classical word, the " stem " within the meaning of this Article, shall be that portion of the name that shall be first selected to receive the suffix "-IDAE," "-INAE," or any other patronymic (or equivalent) suffix intended to convert that generic name into the name of a group higher than genus.

(*b*) In case the name or the last element of the name consists of a definite classical noun, the " stem " as here used shall be understood to consist of the genitive form of that noun without its case ending.†

I believe that these misspellings should be weeded out as rapidly as discovered, that they should not be looked upon as emendations, that no attention should be paid as to whether they have been discovered in the past and corrected or have yet to be, and that the suprageneric name should date from its original introduction, even though misspelled.

Most of the difficulty has in the past arisen in the case of words with increasing genitives, or more exactly, words in which the nominative is a contracted form. It would be helpful to list a few of the most persistent trouble-makers, so that authors could be on their guard. For example (by transliteration to Latin) *stoma*, the stem of which is *stomat-*.

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*Exception occurs in the case of some vowel stems. But this need occasion no difficulty, as the insertion of a practical provision into Article 4 will take care of these cases and greatly simplify the family name formation for non-classicists.

†This will not always be the classical form of the stem, particularly in the case of vowel stems, but it will be that part, form, or contraction of the stem that the ancients would themselves have used in combination with a suffix commencing with a vowel. It will therefore give, very simply, the results originally sought by Article 4. It has the great advantage of simplicity, for the form of the genitive of every classical noun is shown in the usual lexicons, and all that has to be done is to drop the case-ending.

Para. **11**

I concur emphatically that the person who erects a group higher than genus has the choice of the type genus. Any other course is contrary to all principles and definition of " type " for the latter *must* be stable and can not be allowed to fluctuate.

Para. **12**

A supra-generic group (below the rank of suborder) must be considered to have been established, and, in consequence, its name to have been published as from the date when a Latin (or Latinized) term was first applied to a given group of genera, irrespective of the termination then employed for the term so used provided that the term was based on the name of some one of the contained genera with the addition of some suffix, irrespective of what suffix was employed. The tribe, subfamily, family and all other categories (whether officially recognised or not) based on a contained generic name with some added suffix are and must be nomenclatorially co-ordinate. Therefore, when a suprageneric group name is established, tribe, subfamily and family are all potentially established, and it is solely a matter of subjective taxonomic judgment which rank may best be assigned the suprageneric group, and this rank may be expected to be subject to continual fluctuation.

I have always thought that some Latin suffix should be required in order to consider a group established. However, if it will tend to create harmony and the French are strongly of the opinion that vernacular terminations should be recognised (for the purpose of establishing publication), I would agree, but as a somewhat undesirable compromise.

Para. **13**

As stated above, the *rank* accorded a suprageneric group is purely subjective. Whenever one is established, a potential family, even superfamily (but see later qualification) is founded. It should make no difference in principle then what termination was applied or when. Nomenclatorially, the essential act has been performed when the suprageneric group has been established, just as a potential genus has been established when a subgenus has been founded.

I see no advantage and much disadvantage from dividing our practice as before or after a given date, or as before or after its use of a particular suffix.

It is much more difficult to trace the bibliographical history of a family name than of a generic name. Let us examine the following hypothetical case as illustrative of one of the difficulties that the suggested course (adopting suprageneric names prior to 1950 as having family equivalency, regardless of termination but after that date not until they are established with the suffix " -IDAE.")

The group ACHILIDES was established in 1871, but not until 1903 was the form ACHILIDAE used and (according to the Secretary's plan) the family established. Meanwhile it had won general recognition as a tribe, subfamily or family under one or another termination. Let us now assume that in 1900 a small and little known group was split off as a family TROPIDUCHIDAE but that this has since been placed by all authors as a tribe of ACHILIDAE. Nevertheless, we would now have to change ACHILIDAE to TROPIDUCHIDAE, the senior subjective synonym.*

*This is a misunderstanding of my suggestion.—F.H.

Para. **14**

I strongly concur with the Secretary's recommendation which may be expressed by the following general statement of principle: A suprageneric group with a name based on the name of a contained genus is established when its name is first published, regardless of whether the name employed for its type genus is a valid name, or a junior homonym of another name, or an objective junior synonym of an earlier name. In case for either of these reasons the name of the type genus is changed, the name of the suprageneric group must undergo corresponding change, but the type genus is only nominally, not actually changed, since both the names applied to it are objective synonyms.

Para. **15**

I think that we must recognise a name given to a tribe, subfamily, family or any other category employed for a suprageneric group based on the name of a contained genus as co-ordinate (later exception for superfamily), the subjective question of rank having no nomenclatorial significance. With this in view, I should prefer to see the principle established (just in reverse of the Secretary's proposal) that, if the name of the type genus is thought to be a subjective junior synonym, the type genus of the suprageneric group actually remains the same nominal genus, but that any one so holding shall employ the senior subjective synonym for the valid name of the genus and for the basis of the valid name of the suprageneric group. Then I should add that, where specialists in the group concerned desire to retain the suprageneric group name based on the junior subjective synonym, especially where names frequently ranked as family are concerned, they may apply to the Commission for decision. The only great difference is that it would make it normal to have the group name follow along with a contained genus, and would probably result in less applications for relief to the Commission.

Para. **16**

In case two suprageneric groups of the same rank are united, I strongly agree with the Secretary that the older synonym among the two suprageneric names, subjective or objective, must prevail, and that the date of the founding of the names of the type genera has no relevance.

Para. **17**

The date of the founding of some genus within a suprageneric group has no possible bearing upon the type genus of that group, despite widely held misapprehension on that point formerly held.

Para. **18**

Apparently I do not understand the purport of this paragraph, for it is not apparent to me what relationship the date of the establishment of a type species for a genus has to the date of establishment of a suprageneric group. Let us consider an example with the results that would ensue: The great family ICHNEUMONIDAE with more than a thousand genera and many contained tribes, itself dating among the earliest of family names has as its type species the genus *Ichneumon*. The type species of *Ichneumon* was not finally determined until 1935 when the Commission under its plenary powers set aside all prior type designations and arbitrarily selected a type species to conserve the ancient

use of the name. If the suprageneric names ICHNEUMONINI, ICHNEUMONINAE, ICHNEUMONIDAE and ICHNEUMONOIDEA are to date only from 1935, then they must all be superseded by the names of other suprageneric groups based on a genus the type species of which was settled earlier.

The family name BRACONIDAE was one of the earliest used in the Order Hymenoptera, is large and very important. There are many subfamilies. The type of the genus *Bracon* Fabricius, 1804, was not established until 1825 by designation of Curtis. Meanwhile the type species of the type genera of other subfamilies, groups that came into use much later, had been established. Under the suggested procedure we would have to set aside BRACONIDAE and replace it with a family name based on the included subfamily the type genus of which was the first to have a type species fixed, as perhaps ALYSIINAE as based on *Alysia* Latreille 1804, the type species of which was established at that date by reason of its being the only included species. (I have omitted as immaterial the fact that the type species of *Bracon* was also set aside by the Commission in 1935 and a new type species designated.)

Para. **19**

I agree that the suggestion of first reviser, brought forward but rejected by the Secretary, is very objectionable.

As previously indicated, the plain and simple course seems to me to be the following :—

(1) A suprageneric group was founded when a group name was published, based on one of the contained generic names, with whatever added suffix. It makes no difference whether the position or form of the name to which the suffix was added was the classically correct stem, or not.

(2) The rank assigned the suprageneric name has no nomenclatorial significance, nor does any change of rank affect the date of establishment of the name.

(3) In a manner similar to the case of all generic names, the suprageneric name takes precedence over all junior synonyms, subjective or objective. (An objective junior synonym is a suprageneric name based on an objective junior synonym of its type genus. A subjective junior synonym is a suprageneric name established later with a type genus that is held subjectively to belong to the same suprageneric group), and falls in the presence of senior synonyms, objective or subjective.

I am confident that these principles, if adopted, will result in the least possible changes, taking into consideration tribal, subfamily and family names, because these are the principles that, although not formulated, have most generally actuated authors in dealing with such names.

The Commission must be authorised and prepared to grant relief by making exceptions in the cases (which I am confident will be relatively few) where clearly generally accepted names are threatened.

By far the greatest difficulty has been arising in the case of names the type genera of which are now known to have been misunderstood, the type species actually belonging to a taxonomic group far removed from that to which the name was believed to apply. *All such cases should be brought to the attention of the Commission as rapidly as possible and the Commission should be instructed under its plenary power to set aside the type species of such genera, and to establish type species that will conserve the accepted meaning of the generic names.* This was done at my request in the case of numerous families and subfamilies of Hymenoptera in 1935, and a number of such cases are now before the Commission.

Para. **20**

Agreed.

Para. **22**

Such rare cases can be specially handled under plenary powers, and the Commission should be authorised to do so, when, in its judgment, the case warrants. The authorisation already adopted at Paris seems adequate.

Para. **23**

It would seem to me far better, where such a case arises, to conserve the junior synonymous generic name, under the plenary powers, and thereby to achieve the desired conservation of the family name. The very case brought forward as an illustration emphasises this, for I am convinced that, whatever action the Commission may or may not take, the majority of zoologists will *not* abandon *Chironomus* in favour of *Tendipes*.

Para. **24**

I see no objection to the course indicated, though I should personally be doubtful that a case would ever arise where it would be really desirable to conserve the family name and at the same time *not* to validate the generic name on which it is based.

Para. **25**

Author of a family name. I can only think of this in terms of author of a suprageneric name, with no more regard to rank assigned it than we pay to rank as between genus and subgenus.

It has been very customary to apply the author's name after a tribal, subfamily or family name to imply the author who gave the name a certain rank and certain limits. This must be strictly prohibited in the future and had better be provided for by some such provision as " If it is desired to cite the author who first assigned a particular rank and limit to the suprageneric group, this may be done by adding in parentheses (sense of so-and-so, such-and-such a date)."

Para. **26**

Agreed, on the principle that it is the author of the suprageneric group being cited, not necessarily of the valid name of that group. With this in mind, might it not be simpler to provide that if the author of the suprageneric group used a name for it based on a different generic name, his name be cited but placed in parentheses ?

Paras. **27-28**

Hemming's suggestion " to be rejected as a junior homonym " produces an absolute impasse. The type genus of the suprageneric name is that which gives its stem to the homonymous suprageneric name ; but the genus is not a homonym and its name is valid. To shift the type genus is impossible under the rules, and would nullify the existence of any type. Yet there is only one way to form a suprageneric name under the *Règles* from that generic name. The only escape is by suspension of the rules and plenary action.

The principle is the same throughout all suprageneric categories, and since there are many cases of two or more genera with identical stems, I shudder to think of the number of homonymous names that may exist at tribal level or above that have never been noted.

Para. **29**

Agreed, except that generic names that are not homonyms may produce family names that are identical, ex. gr. *Cyprinus* and *Cyprina*. I think the wording must be based on whether there is any difference in the *stems*, not in the nominatives.

Para. **30**

Agreed, except that (as I have previously stated) I think we must go further, and provide that the regulations must apply to all suprageneric names based on a contained generic name, without any regard whatsoever to the subjective rank assigned to them.

Para. **31**

Very strongly agree that the suffix " -INAE " must be retained for the subfamily.

Para. **32**

Strongly agreed to. The only exception in practice that I can think of is that myrmecologists have for some reason customarily used *Formica* as the type genus of FORMICIDAE, but placed it in the subfamily which they have termed CAMPONOTINAE.

Para. **33**

Strongly agreed to, as far as it goes.

Paras. **34** and **35**

Agreed to.

Para. **37**

Supergenus. I cannot recall where it is in practical use. I have thought of its involving the generic name without suffix (just as does the subgenus) and that while it would never be combined with the trivial name in citing a species, that it would differ from genus only in degree of comprehensiveness, that it would be an aggregate of genera. In citation it would be distinguished from genus only by insertion of the word " Supergenus " in front of it. Would it not be well to postpone rules for this category until demand appears ? But somewhere in the *Règles* should be a list of primary categories from Kingdom

to species, with a statement that it is always permissible to provide intermediate categories by using the primary grades with the prefixes " SUPER- " or " SUB-."

Para. **39**

Agreed.

Para. **40**

Malacologists have almost universally used the termination " -ACEA " for superfamily. However, I have seen at least one instance of a recent author who has adopted " -OIDEA." I concur that " -OIDEA " should be the suffix. I do not like to see each category of this group of names defined in terms of a type consisting of a name of the next inferior rank. Is it not better to let the type genus stand as the type unit for all categories based on it ?

Para. **41**

I agree that, for purpose of priority, a special case must be made of superfamily, apart from other suprageneric names.

Para. **42**

Agreed.

Paras. **43-48**

Agreed to, but with two suggestions :—

(1) that each suprageneric name be entered in the form in which it was first established, with its author and date, followed by its first usage, if any, with each of the authorised suffixes " -INI," " -IDAE," and " -OIDEA," with the author and date of each.

(2) that it might be clearer to entitle the list " Official List of Suprageneric Names in Zoology."

Should not suprageneric name be defined as any name of a group higher than genus based on the stem (or its equivalent) of a contained genus with a suffix added to indicate its rank ? This definition would exclude subordinal and higher names.

Would it not be well to provide that systematists requiring additional levels of classification are at liberty to introduce such additional categories as phalanx, cohort, stirps, legion, etc. at points between genus and suborder, applying the same principles as for other suprageneric names, and to use any suffixes other than those received for tribe, subfamily and superfamily, but that it is not deemed necessary to make special provision for them in the rules.

Subtribe, however, is used so frequently that it should perhaps be included along with tribe. The suffix " -INA " is often employed.

DOCUMENT 3/38

By W. E. CHINA, M.A., D.Sc.

(*British Museum (Natural History), London*)

(1) Extract from a letter dated 20th August 1952

Ref. Z.N.(S.)357

I have also read through *Bull. zool. Nomencl.* Vol. 7, Part 3, dealing with the portion of the *Règles* relating to the formation of family names. Here, although I am in general agreement I am not in favour of the methods of determining the relative status of competing family names embodies in paragraphs 18 and 19. The use of the date on which the type species of the type genus was fixed to decide the priority of a family would completely upset our family names, since many genera have had the type species fixed only within recent years and thus the priorities afforded would be of a very low standard. The first reviser principle of paragraph 19 would, in my opinion, be impracticable. The first reviser is often the worst reviser and there has usually been an alteration in the use of group names which does not finish with the name selected by the first reviser. In my opinion, the Law of Priority should be the guiding principle in the selection of family names. A family name should be the first published group name based on the stem of a valid generic name, irrespective of the type of plural ending used by the author. The family name must date from the publication of this group name and the type genus, of course, is automatically the genus on which the group name is based. Considerable instability of family names has been due to the rule stating that " the name of a family must change when the name of its type genus is found to be a synonym, the family name can be retained since the its nominal type genus is found to be a homonym. When the nominal type genus is found to be a synonym the family name can be retained since the name of its type genus still remains within the family although as a synonym. Unless this system is adopted, family names will be continually liable to change. An *Official List of Family Names*, as proposed in paragraphs **45-48** could validate names of families, whose type genera have become synonyms.

There is one point which I cannot find clarified in your report. That is, to whom should a typical subfamily be attributed, the original author of the family or to the author of the subfamily grouping ? Since subfamilies are co-ordinate with families, I presume that the date of a typical subfamily would be the same as that of the family in which case the author would also be the same. What about the other non-typical subfamilies ? Surely they must be attributed to their respective initiators and given their dates of initiation !

As regards paragraph **38** I suggest the ending " -ARIA " to the stem of the type genus of a supergenus. This ending was used extensively by Distant in his *Fauna of India* volumes for groups of genera below tribal rank.

(2) Extract from a letter dated 20th September 1952

Z.N.(S.)357. New family names based on a generic name substituted for the original type genus which has become a homonym

I agree with you that for purposes of stability the new family name should take the date of the old family name. Thus, RIODINIDAE should take the date of the previously used ERYCINIDAE. I was previously confusing two ideas, the need for a Law of Priority in family names and the need for a family name once established to follow the vicissitudes of its type genus. There is, of course, always the chance in such cases that a new family name based on a substitute generic name may change again because the genus which had priority and should have been selected as the substitute name had been overlooked and some later name used as the substitute. I therefore think that for the purposes of stability, no time should be lost in establishing an *Official List* of all family names.

DOCUMENT 3/39

By K. H. L. KEY

(*Commonwealth Scientific and Industrial Research Organisation, Division of Entomology, Canberra, Australia*)

Extract from a letter dated 4th September 1952

(Note :—The main portion of the above letter was concerned with the problem of the emendation of names, on which a separate paper has been submitted by Dr. Key and which constitutes Document 5/26 of the Copenhagen Series.)

I have studied your proposals in connection with the other six questions referred to you by the Thirteenth Congress, and, in general, I find myself in hearty agreement with them.

DOCUMENT 3/40

By D. LESTON
(*London, England*)

Enclosure to a letter dated 7th March 1953

Editorial Note: *The following note is an extract from a paper by Mr. Leston entitled "The Suprageneric Nomenclature of the British Pentatomoidea (Hemiptera)," which was published in January* 1953 *in the serial publication "The Entomologist."*

In the hope of contributing to the stability of *nomenclature* in this present period of taxonomic fluxion, the group names used in the classification of the British Pentatomoidea have been investigated.

The principle of giving priority to the oldest Latin group name of suprageneric rank has been used but, if the name of the original type-genus has changed (in accordance with the International Rules or *Opinions*), then the group name has been changed accordingly. Horváth (1911) followed the oldest group-name method as opposed to Bergroth's (e.g. 1926) use of the oldest generic name as the root of a suprageneric unit, but Horváth refused to alter a group-name when its type-genus had been relegated to synonymy. Thus he retained PLATASPIDAE Dallas 1851 although Kirkaldy (1909) had shown *Plataspis* Westwood 1837 to be a synonym of *Brachyplatys* Boisduval 1835. (Bergroth's remarks (1906) to the effect that *Plataspis* and *Brachyplatys* were distinct were not based on a study of the genotypes.) Leston (1952) has therefore raised the name BRACHYPLATIDAE in place of PLATASPIDAE ; it is obvious that the original type-genus of the first erector of this suprageneric group has remained unchanged. The alternate name used by many authors, COPTOSOMATIDAE (or COPTOSMIDAE), was based on the oldest generic name.

Schouteden (1906c), on the question of "Asopinae," wrote "Pour être rigoureusement exact il faudrait cependant changer le nom de la sousfamille en Amyotinae, le nom du genre type *Asopus* ayant du être remplacé par *Amyotea*." Therefore Schouteden placed AMYOTINAE on the title-page of his monograph, but unfortunately in brackets after the disqualified name. It is shown by Schouteden's use of Asoparia on a later page that he did not intend to replace ASOPINAE by AMYOTINAE and thus the latter name is proposed below.

The method used here is also used by China (1943) when, showing that *Stenodema* Lap. must replace *Miris* Reuter *nec* F., the type genus of MIRINI Reuter *nec* Hahn, he amends the tribal name to STENODEMINI China. (But China retains BERYTIDAE although pointing out that the type genus, *Berytus* F., is a synonym of *Neides* Latreille.) Similarly, Carvalho follows the same practice throughout his recent revision of the MIRIDAE (1952).

Group names ending in -ides have been accepted as being Latin and both Leach (1815) and Billberg (1820) intended such ; unfortunately, the same ending when used by Amyot and Serville (1843) is undoubtedly French and

their names thereby invalid. Such names used by them as Stiretrides (p. 74), Phleides (p. 115) and Megymenides (p. 181) are obviously vernacular. Sailer (1945) bases a subfamily name, EUCORIINAE, on a vernacular name, Eucoriens Mulsant and Rey (1866) but, as pointed out by McAtee and Malloch (1945), Eucoriens is invalid ; Sailer's action validates it as EUCORIINAE Sailer. Van Duzee (1917) was inconsistent ; e.g. he accepted Sciocorides A. & S. but not Sehirides A. & S.

To bring the work into line with current usage, the equivalent names used by Van Duzee (1917), Stichel (1935) and Poisson (1951), as well as those of China (1943), are given ; the last-named classification has, for convenience, been followed except for one change. Although no official ruling has been given by the Commission it is anticipated that *the original author of a group shall be cited at no matter what level*—tribal, subfamilial or familial—*the group is subsequently placed* by taxonomists. Thus Pentatomoidea Leach, Pentatomidae Leach, Pentatomini Leach are considered here as correct. The old method seems to have been arbitrary : e.g. Stichel (1935) cites Miridoidaria Stichel, Miridae Dohrn 1859, Mirinae Reuter 1910, Mirini Reuter, 1875 ; the oldest group name, Mirides Hahn (1831), not originally conceived as vernacular, has been overlooked by Stichel and it would appear that Hahn should be cited as the author of all these groups.

The advantages of the method used here are : (1) its simplicity ; (2) the retention of the original author's name through all the later vicissitudes splitting or lumping, upgrading or downgrading, of the group during the subsequent development of taxonomic theory and practice ; (3) the strict parallel with the rules on generic nomenclature ; (4) its compliance with the International Rules as far as they have been formulated on the subject of group-names.

PURCHASED
21 JUL 1953

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Notices to Subscribers

(1) With the publication of the present Part (Parts 6/9), volume 8 is complete, except for one concluding Part, containing the Title-Page, Table of Contents and the Index. It is hoped that this Part will be published in the late summer of the present year.

(2) Owing to the need for concentrating upon the preparations for the Colloquium on Zoological Nomenclature to be held at Copenhagen at the end of July 1953 and for the meeting of the International Commission on Zoological Nomenclature to be held immediately thereafter, it has been necessary temporarily to stop work on the current volume of the *Bulletin of Zoological Nomenclature*. A considerable amount of matter is however in an advanced state of preparation, and it is hoped that further Parts (commencing with Part 6 of volume 9) will be published shortly after the Copenhagen meetings.

(3) The index to volume 6 has now been completed and is in the press. It is hoped that it will be possible to publish this (with the Title Page and other Introductory matter) in a concluding Part (Part 12) within the next few weeks.

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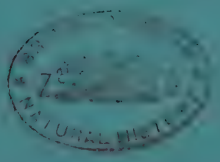
THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature



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BULLETIN OF ZOOLOGICAL NOMENCLATURE

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EDITORIAL NOTE

The present Part contains the four concluding papers in regard to the Cases dealt with in the present volume which were considered by the Colloquium on Zoological Nomenclature, Copenhagen, 1953, but which were received too late for publication prior to the meeting of the Colloquium. These documents were submitted to the Colloquium in mimeographed form. They are now published, in order to complete the record of the documentation submitted in regard to the foregoing Cases.

These documents are the Reports prepared by the Secretary to the Commission in response to the invitation addressed to him by the Thirteenth International Congress of Zoology, Paris, 1948.

ADDENDUM

REPORTS SUBMITTED TO THE COLLOQUIUM ON ZOOLOGICAL NOMENCLATURE, COPENHAGEN, 1953, BY THE SECRETARY TO THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE ON THE THREE MAJOR NOMENCLATORIAL PROBLEMS DISCUSSED IN THE PRESENT VOLUME

CASE NO. 1

The problem of securing greater stability in zoological nomenclature

(*continued from page* 108)

DOCUMENT 1/59

FIRST REPORT by FRANCIS HEMMING, C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

Stabilisation of Zoological Nomenclature, Part 1: The Suggested Law of Prescription

(Circulated to the Colloquium on Zoological Nomenclature on 29th July, 1953 as Paper Z.N.(C.) 16)

Origin of the present investigation: At the Paris (1948) Congress two proposals were brought forward in favour of the introduction of a Law of Prescription which would limit the operation of the Law of Priority in certain respects in the interest of maintaining names currently in common use; the first of these proposals was brought forward by Lemche (*Copenhagen*), the second by Bonnet (*Toulouse*). Both these proposals were published in 1950 in vol. 3 of the *Bulletin of Zoological Nomenclature*. The preliminary discussion of this problem at Paris both showed the complexity of the problem and brought to light also considerable differences of opinion. It was accordingly decided (1) to defer the consideration of this matter until the Copenhagen (1953) Congress, (2) to broaden the issue by including in it all other possible ways of promoting stability in nomenclature, and (3) to invite me, as the Secretary to the International Commission, to undertake an investigation, in conjunction with interested zoologists, and to submit a full report to the Copenhagen Congress.[1]

2. Consultations made and advice sought: In March 1952 the Trust published a paper (*Bull. zool. Nomencl.* **7**: 148-188) in which I gave a survey of the various suggestions which had been made and of the issues which appeared to be involved and invited interested zoologists to furnish statements setting out their views as to the action which it was desirable should be taken at the Copenhagen Congress. The documents received in response to this appeal, together with various documents previously received by the Commission, have since been published in Parts 1/3 of vol. 8 of the *Bulletin of Zoological Nomenclature*.[2]

[1]See 1950, *Bull. zool. Nomencl.* **4**: 234.
[2]Pp. 5-108 of the present volume.

3. The present separate treatment of the proposal for the recognition of a Law of Prescription : In my 1952 survey I was inclined to take the view that the main issue in the field of stability was whether some form of a Law of Prescription should be introduced or whether, in place of this course, various other means there enumerated should be adopted to promote stability in zoological nomenclature. Looking back, it appears to me that, while from a practical point of view the foregoing might prove to represent the broad choice confronting the Colloquium, it by no means follows logically that this is the case, for, as pointed out in a number of the communications received in response to the 1952 appeal, it would be possible both to adopt some form of a Law of Prescription and, in addition, to adopt some or all of the various other measures which have been suggested. In view of the novel character of the proposal to recognise a Law of Prescription in the *Règles* and of its intrinsic importance, I have come to the conclusion that it might be convenient to the Colloquium to have before it a short Report summarising the various forms in which this proposal, or something substantially equivalent to it, have been put forward. In all there are ten of these proposals. The authors of these schemes, arranged in the order in which they were received by the Commission, are : (1) Bonnet ; (2) Lemche's 1948 proposal ; (3) an organised group of Austrian zoologists ; (4) Ferrière ; (5) Dammerman ; (6) Daniel ; (7) Blake ; (8) Kiriakoff ; (9) Mayr ; (10) Bradley. Apart from Bradley's scheme which was received in 1952, all the schemes noted above were received at various dates between 1948 and 1951. These schemes are summarised in the following paragraphs.

4. The Bonnet proposal : The text of Professor Bonnet's proposal was published in 1950 (*Bull. zool. Nomencl.* **3** : 177) and has been included in the Agenda for the Colloquium as Document 1/3.[3] His proposal, submitted early in 1948, is that there should be inserted in the *Règles* a provision that a generic name or a specific trivial name which has remained unused for a considerable time (" durant des nombreuses années ") shall not, on grounds of priority, be substituted for a name which has been in use for a long time (" depuis longtemps utilisé "). Under this proposal difficult cases would be left to specialists to settle among themselves.

5. The Lemche proposal of 1948 : The proposal brought forward in 1948 by Dr. Henning Lemche (on his own behalf and on that of a large group of Scandinavian zoologists) was published in the *Bulletin* in 1950 (*Bull. zool. Nomencl.* **3** : 158-161) together with the names of the supporting zoologists ; and the operative portion of this proposal has been included on the Agenda for the Colloquium as Document 1/7[4]. Under this proposal it was suggested that there should be inserted in the *Règles* a provision that a generic name or specific trivial name published before 1850 and not used in scientific literature since 1st January 1850 is not to be rejected in favour of such other name as has been in general use for that genus or species since that date. A revision of this proposal was submitted by Lemche in 1952. See paragraph 13 below.

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[3]See page 5.

[4]See page 8.

6. The view put forward by the Austrian zoologists who have submitted statements on this subject : The group of Austrian zoologists who have submitted representations in support of the Principle of Prescription favour the recognition of the principle that a name which has been consistently applied to an animal should not be displaced in favour of an older name solely on account of technical nomenclatorial considerations. This view was put forward in a letter addressed to the Thirteenth International Congress of Zoology at Paris in 1948 in a communication, the text of which has since been published in the *Bulletin* (1950, *Bull. zool. Nomencl.* **5** : 77-78). See also Document 1/9 in the Agenda for the Colloquium.[5]

7. The Ferrière proposal : The proposal submitted to the Commission by Dr. Ferrière (and placed also before the Ninth International Congress of Entomology at Amsterdam in 1951) is reproduced as Document 1/13 in the Agenda for the Colloquium.[6] Under this proposal, where a species remains unidentified for more than one hundred years, the name applied to that species (1) would be suppressed altogether if the original type specimen was no longer available, and (2), if the type specimen were to be available, would need to be redescribed in its appropriate modern genus before it could be accepted as an available name. Ferrière recognised that the crux of his proposal was the date to be assigned for purposes of priority to a hundred-year-old species redescribed in this way. On this critical issue he was able to suggest only that it should form the subject of discussion among specialists.

8. The Dammerman proposal : The proposal put forward by the late Dr. Dammerman—for which see Document 1/14 on the Colloquium Agenda[7]—is of a general character, although it was put forward specifically only in relation to the names of insects. The idea was that a definite start should be made towards preserving specific trivial names belonging to two important classes, namely (*a*) names of species which are the type species of genera which, in turn, are the type genera of families, and (*b*) names of species of economic importance. The proposal was that, under a system of co-operation to be established among leading specialists, a specified number of names of the foregoing classes should be studied annually, that the results of those studies should be published in the *Bulletin of Zoological Nomenclature* and that, if after the expiry of a period of one year from the date of such publication, a given name received a two-third vote, that name would thereupon become fully protected against rejection on any ground.

9. The Daniel proposal : The proposal submitted to the Commission by Dr. Franz Daniel is reproduced as Document 1/25 on the Colloquium Agenda.[8] Dr. Daniel's scheme is similar generally to the Dammerman proposal in that it contemplates progress being achieved through proposals to be submitted by relatively small groups of specialists in particular groups of the Animal Kingdom.

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[5]See page 13.
[6]See page 20.
[7]See pages 21-22.
[8]See pages 46-47.

10. The Blake proposal : Blake (Document D 1/31 on the Colloquium Agenda[9]) put forward a suggestion in favour of the insertion in the *Règles* of a Statute of Limitation. Under this proposal which, in his covering letter Blake stated was, in part, derivative from ideas suggested by Mayr, the exclusive and uncontested application of a given name to a particular species for a period of fifty years or more would be deemed to constitute conclusive evidence that the name was correctly so applied, provided that the name had been used at least three times during that period. Similarly a name would be protected where it had been applied for 70 years or more to a given species, even if on not more than three occasions during the 70-year period the name in question had not been so called.

11. The Kiriakoff proposal : Kiriakoff is opposed to the introduction of a Law of Prescription ; he proposes, however, a much more drastic scheme (see Document D. 1/32 on the Colloquium Agenda[10]), under which after a date to be specified no further changes in the names of genera or species would be permitted and any book published before that date which contained names for genera or species at present known by later names would be treated as non-existent for nomenclatorial purposes.

12. The Mayr proposal : In Document 1/45 on the Colloquium Agenda[11] Mayr puts forward a proposal which resembles the Blake proposal (paragraph 10 above), which latter (as already explained) is stated by its author to be in part derived from ideas suggested by Mayr. Under this proposal three new categories of status would be recognised for names : (1) that of the " *nomen conservatum*," to which would belong any name expressly preserved by the Commission and placed on either of the *Official Lists* ; (2) that of " *nomen conservandum*," to which would belong (i) all names on the *Official Lists* other than " *nomina conservata*," and (ii) any name in current use for the conservation of which a petition to the Commission is pending ; (3) that of " presumptive ' *nomen conservandum* ' " to which would belong (i) the name of any genus which is the type-genus of a suprageneric category ; (ii) any name (otherwise available) which has been used exclusively or virtually exclusively for a period of 50 years or more, provided that the name in question has been used at least ten times in the literature, or for a period of 30 years where the name in question has been used 100 times or more during the period. Under this scheme the Law of Priority would not apply to " *nomina conservanda* " or to " presumptive *nomina conservanda* " in such a way as to disturb usage without prior reference to the Commission. It would be the duty of zoologists to bring such cases to the attention of the Commission.

13. The present Lemche proposal : Lemche's present proposal is given in Document 1/46 on the Colloquium Agenda.[12] This scheme differs in form from any of the others that have been proposed in that, under it, the burden of searching the literature for the purpose of finding cases where a practically

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[9]See pages 56-57.
[10]See page 58.
[11]See pages 76-77.
[12]See page 78.

unknown name has been used, would be thrown upon the author who wished on grounds of priority to use some name (name " A ") in preference to the later name (name " B ") currently in use. Such an author would be required not merely to show that the names " A " and " B " belong to the same species and that the name " A " is older than the name " B," but also that the name " A " has been used for the species concerned in preference to the name " B " at least (—) times during a period of (—) years from the time at which the author concerned desires to substitute the older name " A " for the currently accepted name "B." This scheme would base prescriptive right for the name " B " upon the non-usage of the name " A "—or the usage of that name on not more than (—) occasions—in a specified recent period. The length of the period to be selected and the number of occasions on which, during that period usage of name " A " occurred, would be a matter for separate discussion.

14. The Bradley proposal : In Document 1/50 on the Colloquium Agenda[13] Bradley, while expressly supporting the idea of a Law of Prescription, puts forward a scheme which is in fact closely allied to that advocated by Mayr (paragraph 12). Under this proposal a name which has been " virtually exclusively in use " for a specified period, or has been so used in a specified number of publications, shall be presumed to be a " *nomen conservandum* " ; that is, it shall not be permissible for a zoologist who observes what he deems to be a nomenclational defect in such a name to reject it under any provision of the *Règles*, it being his duty in such a situation to report the case to the Commission. Such cases would be decided by the Commission under its ordinary—that is, not under its Plenary—powers and, where the Commission considered that the name qualified for conservation, would place it on the appropriate *Official List*.

15. Support received for the idea of inserting in the " Règles " a provision embodying a Law of Prescription : It will be seen from the foregoing particulars that the idea of inserting in the *Règles* a provision embodying a Law of Prescription in one form or another has attracted a substantial measure of support. In addition to the authors of the actual schemes put forward this idea is supported also, though with qualifications, by : (1) The Nomenclature Committee of the American Museum of Natural History, New York (Document D.1/41 on the Colloquium Agenda[14]) pointed out that a Law of Prescription would involve an exhaustive search into the entire literature, a search which the committee regarded as " probably an impossible task." The Committee did not however rule out the possibility of devising a scheme under which a name might acquire a prescriptive right where it had been used with moderate consistency in comprehensive treatments of the group concerned, by calling in aid some Statute of Limitations. The Committee added that Mayr (one of its members) had suggested that the period for acquiring a prescriptive right might be reduced from 100 to 50 years and that usage in synonymies should be disregarded. (2) China (Document

[13]See pages 86-87.
[14]See pages 71-72.

D. 1/52)[15] considers that a properly worked-out Law of Prescription would be a great help in stabilising nomenclature, but that (*a*) the period for acquiring a prescriptive right should be set at 40 years, and (*b*) that the new provision should not be retrospective to the period lying behind the date on which it was adopted ; in other words, the new rule would apply only to cases where for 40 years after its introduction the name in question had been applied in a uniform manner.

16. Opposition to the recognition in the " Règles " of the principle of Prescription : While, as noted in the preceding paragraph, considerable support has been expressed for the proposed incorporation in the *Règles* of provisions embodying a Law of Prescription or its equivalent, it must here be noted that this proposal has excited also considerable opposition. It is rather difficult to analyse the grounds upon which various authors have based their objection, but it is clear that some hold that such a scheme would be impracticable, while others regard it as unsound in principle, infringing the Law of Priority without securing a sufficient countervailing advantage. Some who have expressed opposition to the idea of a Law of Prescription have taken the stand that the promotion of stability in nomenclature could be achieved more satisfactorily by the adoption of the other measures suggested in my 1952 paper and, perhaps because of the form in which the questions at the end of that paper were drafted, have not made it clear whether they would in all circumstances be opposed to the adoption also of a Law of Prescription. Those who have expressed themselves as being opposed, for one reason or another, to such a provision include : (1) di Caporiacco (D. 1/5[16]) ; (2) Holthuis (D. 1/23[17]) ; (3) Hopkins (D. 1/24[18]) ; (4) Arkell (D. 1/29[19]) ; (5) Dymond (D. 1/30[20]) ; (6) Kiriakoff (D. 1/32[21]) ; (7) Cabrera (D. 1/36[22]) ; (8) Keen & Muller (D. 1/40[23]) ; (9) Wright (D. 1/43[24]) ; (10) Lochhead (D. 1/47[25]) ; (11) Hall (D. 1/48[26]) ; (12) Townes (D. 1/49, Annexe 1[27]). To the foregoing must certainly be added the Nomenclature Discussion Group, Washington, D.C. (D. 1/8[28] and D. 1/42[29]), for, although in neither of the statements furnished by the Group is opposition to the recognition of the Principles of Prescription expressly stated, the purpose of both statements is to circumscribe and limit the use of the Commission's Plenary Powers, an object which would be wholly inconsistent with support for the relaxation of the Priority Principle through the incorporation into the *Règles* of a provision recognising Prescription.

(Signed) FRANCIS HEMMING,
Secretary of the International Commission on Zoological Nomenclature.

Temporary Secretariat of the Commission,
Hotel Excelsior,
Copenhagen,
Denmark.
29th July, 1953.

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[15]See pages 97-98.
[16]See page 7.
[17]See page 44.
[18]See page 45.
[19]See page 55.
[20]See page 55.
[21]See page 58. See also paragraph 11 of the present Report.
[22]See pages 66-67.
[23]See page 70.
[24]See page 75.
[25]See pages 79-80.
[26]See page 80.
[27]See page 81.
[28]See pages 9-12.
[29]See pages 73-75.

DOCUMENT 1/60

SECOND REPORT by FRANCIS HEMMING, C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

Stabilisation of Zoological Nomenclature, Part 2: Possible measures other than the adoption of the Principle of Prescription

(Circulated to the Colloquium on Zoological Nomenclature as Paper Z.N.(C.)17 on 29th July, 1953, simultaneously with the Secretary's First Report (Document D.1/59))

In my immediately preceding Report (Paper Z.N.(C.)16) I have given a summary of the suggestions which have been made in favour of the incorporation into the *Règles* of provisions recognising the Principle of Prescription. I dealt with this subject separately in the foregoing Report, mainly because the documentation relating to it was much more voluminous than that relating to any other matter in the group of proposals expressly concerned with the promotion of stability in nomenclature. The purpose of the present Report is to give a list of the proposals which have been received in regard relating to the promotion of nomenclatorial stability, other than the suggested recognition of the Principle of Prescription :—

(1) Proposed addition of a Preamble to the *Règles* making it clear that their object is to promote uniformity and stability in zoological nomenclature.

(2) Proposed rewording of the Plenary Powers provisions to make it clear that the object of the grant of those Powers is to promote uniformity and stability in zoological nomenclature.

(3) Proposed establishment of an *Official Index of Rejected and Invalid Zoological Publications*.

(4) Proposed suppression of unrecognisable (and unrecognised) " nomina dubia."

(5) Proposed introduction of means for determining the locality to be accepted for nomenclatorial purposes as the original locality of the nominotypical (nominate) subspecies of a polytypic species in cases where the information contained in the original description was insufficiently precise or was incorrect or where no information on this question was given by the original author.

(6) Question whether provisions similar to those suggested in (5) above are desirable for the purpose of determining for nomenclatorial

purposes the situation in which the type material of fossil species was obtained when the original description contains no, or inadequate, information on this subject.

(7) Question in relation to the determination for nomenclatorial purposes of the host species of a parasitic species in situations similar to those indicated in (6) above.

(8) The development of the use of the Commission's Plenary Powers.

(9) Development of the "*Official List*" system as an agency for the promotion of stability and uniformity in zoological nomenclature.

2. In connection with the matters enumerated above, it should be noted that no proposals have been brought forward for the extension of the Commission's Plenary Powers, it being considered that the existing provisions are adequate for the purpose of promoting stability and uniformity in nomenclature. It must be noted, however, that among the papers submitted there is a proposed (Document 1/42[30]) submitted by the Nomenclature Discussion Group, Washington, for restricting the Plenary Powers within a narrower field than at present.

(signed) FRANCIS HEMMING,

Secretary to the International Commission on Zoological Nomenclature.

Temporary Secretariat of the Commission,
Hotel Excelsior,
Copenhagen,
Denmark.

29th July, 1953.

[30]See pages 73-75 also pages 9-12.

CASE NO. 2

The question whether " neotypes " should be recognised in the " Règles " as a category of type specimen

(*Continued from page* 166)

DOCUMENT 2/38

REPORT ON THE QUESTION OF THE RECOGNITION OF NEOTYPES AS A CATEGORY OF TYPE SPECIMEN

By FRANCIS HEMMING, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Circulated to the Colloquium on Zoological Nomenclature on 27th July, 1953, as Paper Z.N.(C.)15)

Origin of the present investigation : The present investigation was undertaken in response to a request made at the Paris (1948) Congress (see 1950, *Bull. zool. Nomencl.* **4** : 192-193).

2. Consultations undertaken : In March 1952 the Trust published (*Bull. zool. Nomencl.* **7** : 131-147) a paper in which, as Secretary to the Commission, I reviewed the problems which, as it seemed to me, were involved in the proposal that " neotypes " should be recognised as a category of type specimen and appealed to specialists for advice. The documents received in response to this appeal, together with certain others previously submitted to the Commission, have been included in the Colloquium Agenda as Documents D.2/1 to D.2/37 (*Bull. zool. Nomencl.* **8** : 109-168).

3. The main issues involved : There are two main issues involved, namely : (1) Is it desirable in any circumstances that " neotypes " should be recognised as a category of type specimen ? If the answers to the above question is " yes," the second and equally important question is : (2) Shall the creation of definitive and authoritative neotypes be limited to cases where specialists in the group concerned are of the opinion that a neotype (or some equivalent mechanism) is necessary to put an end to doubts as to the taxonomic species to which a given name is applicable by definitely attaching the name in question to a specimen which corresponds as closely as possible both with the original description and with the current interpretation of that description ? Or alternatively, is the creation of neotypes to be permitted wherever the holotype or lectotype is missing and, in the latter case, where there are no syntypes available for selection as the lectotype ? Clearly, if the answer to the first of these questions is in the negative, the whole problem disappears.

If, however, this question is answered in the affirmative, the choice between the second pair of alternatives becomes one of prime importance, for on the decision taken on this question will turn the whole of the provisions to be adopted. On the one hand, if the creation of large numbers of neotypes is to be permitted, the provisions to be inserted in the *Règles* can be of only the most general character and can be little more than recommendations to authors as to the principles which they should follow in creating neotypes. If on the other hand, the decision is in favour of the adoption of provisions permitting the establishment of neotypes only in exceptional circumstances in relation to well-known names in common use where specialists feel that this is necessary for the purposes of their taxonomic work (i.e., to prevent confusion and to promote uniformity), the provisions to be inserted in the *Règles* could be given a precise character and could be accompanied by safeguards on such matters as publicity, consultation, preservation and location of, and access to, neotypes, which would be impracticable if the number of neotypes to be created was large and if it were to be possible for any worker to create neotypes at his sole discretion.

4. Answers received (documents published in volume 8 of the "Bulletin"): In a consultation undertaken by correspondence—as contrasted with a round-table discussion—it is often difficult, if not impossible, to tabulate the replies received in cases, such as the present, where overlapping possibilities are involved. In the present case there is a minority which is opposed to the grant of any official recognition of neotypes and a majority which is in favour of such recognition. It is clear, however, that some part of the body of workers who make up this majority would join the opponents of neotypes, unless the scheme were one which provided for the creation of only small numbers of neotypes under rigorous safeguards. In a considerable number of cases, however, the comments submitted are based upon the assumption that basically the scheme should be one under which it would be open to any worker to create neotypes without resort to any external authority. Inevitably, therefore, the safeguards suggested are in many cases of very meagre description.

5. Questions relating to safeguards and other matters on which decisions will be needed (1) if a decision in favour of neotypes is taken and (2) as soon as it has been decided whether the scheme is to be one under which neotypes were to be created only in small numbers and under rigorous safeguards or one under which individual workers would be free to create unlimited numbers of neotypes at their own discretion: For the reasons explained above, it seems to me to be best to leave the various comments which have been received (and which have been published in Volume 8 of the *Bulletin*) to speak for themselves, and here to do no more than list the questions which have been raised, while noting that the answers to be given to those questions will inevitably differ greatly according to whether neotype-creation is to be left in the hands of individual specialists or is to be placed under the authority of the International Commission:

(A) *Scope of the Scheme :—*

(1) Shall the creation of neotypes be limited to cases where the holotype and/or lectotype or, if there never were such, the syntypes of a species are known to have been lost or destroyed ?

(2) Shall the creation of neotypes be permissible also where the holotype or lectotype is so damaged or incomplete as, in the opinion of specialists, to render unrecognisable the taxonomic species represented by the nominal species concerned ?

(3) Shall the creation of neotypes be extended to cover also those nominal species which were named from figures or descriptions published by previous authors and thus never possessed any type specimens ? (This is the suggestion made by Kevan in Document D.2/11 (: 119) ; it is also discussed in Document D.2/29 (: 155) by Bradley who considers neotypes necessary in such cases.)

(B) *Relation of neotypes to surviving type material :—*

(4) If neotypes are to be permitted only where the type material is believed to have been lost or destroyed, what is to be the status of the neotype if later the type material or some of it is rediscovered ? In such circumstances shall :

(*a*) the neotype prevail (in the interests of continuity and stability) or the rediscovered type material ?

or

(*b*) the neotype at once lose its status as the unique standard of reference for the identification of the nominal species concerned ? (If so, what is the advantage of establishing neotypes ?)

(C) *The problem of safeguards against the abuse of the neotype system :—*

(5) Shall the rules provide that no specimen is eligible for selection as a neotype unless it is (or, prior to being so selected, becomes) the property of a museum or other scientific institution or be placed on permanent loan in the collections of such a museum or institution ?

(6) Is it desirable that there should be rules which would prevent the development of a scramble by museums to accumulate neotypes for their institutions, without regard to the moral claims of other institutions ? For example, where types have been destroyed through an outbreak of fire at some museum, should that museum have a prior right to neotypes to be created in replacement of its destroyed types—in cases where that museum possesses material suitable for designation as neotypes ? By what means are such problems to be resolved ?

(7) Whatever value neotypes may possess would, it is suggested, be lost if the scheme were so loose that it would be possible for two or more workers to set up neotypes independently in different parts of the

world. How is this to be avoided (see (15) below) ? Or is this anomaly to be tolerated by introducing a rule that the principle of priority shall be applied for determining the relative status of competing neotypes ?

(8) Should it be made an essential condition for the creation of a neotype that the institution in which it is deposited (see (5) above) is one which can be reasonably relied upon to take proper care of neotypes confided to its care and to grant reasonable access to those neotypes to serious students ?

(9) Should it be made an essential condition to the establishment of a neotype that either the author establishing the neotype should publish a full description and/or figures of the specimen which he selects as such or should give a reference to a paper containing such a description and/or figure ? If, for example, an author was required, when establishing a neotype, to do no more than give an " indication " of the meagre kind accepted under Proviso (*a*) to Article 25 for the description of a new species, what would be the value of the neotype so created ?

(10) It is assumed that everyone will agree that, if neotypes are to be given official recognition, the specimen selected to be the neotype should normally agree as closely as possible with the original description of the species concerned, e.g., in regard to such matters as sex, size, colouring, etc., though there may be cases where the difficulty of establishing the species to which a name in common use applies arises not only from the fact that the types are lost, but also from the fact that the description appears to have been based upon an aberrant, immature, or otherwise unrecognisable form. If the above represents the general view, what safeguards are desirable and necessary in order to ensure that a specimen selected as a neotype conforms with the above requirements ?

(11) Should a neotype normally be a specimen obtained from the original locality and/or, in the case of fossil species, the same horizon, as that from which the original material was described if this is known, provided that there is no reasonable doubt as to the accuracy of the type locality and/or type horizon cited in the original description ? Would the actual locality and/or horizon from which the neotype was obtained automatically become the type locality of the species ? This is a question of importance in polytypic species where it is necessary to consider the relationship of the nominotypical (or nominate) subspecies to other nominal subspecies established for the nominal species concerned.

(12) It is assumed that the selection of neotypes by an author without adequate consultation with fellow workers would be regarded as reprehensible and not calculated to promote stability in the nomenclature of the group concerned. What measures should be

taken to ensure that adequate notice should be given to fellow workers before a neotype is established and that, before the establishment of a neotype, adequate regard has been paid to comments received from fellow workers during the period of notice ?

(13) If it is granted that a definite period of notice should be given by an author proposing to establish a neotype, what should be the length of the period and by what means should this notice be given ?

(14) Should it be made an essential requirement that, where the description of a species or its type locality has been more closely defined through restrictions by subsequent authors, an author proposing to establish a neotype for that species should be at liberty only to select as the neotype a specimen which conforms with those restrictions ?

(15) Is it possible to provide adequate safeguards for dealing with the matters enumerated in (5) to (14) above if the establishment of neotypes is to be left entirely to the discretion of individual workers or can those safeguards be provided only if a neotype selection requires the final approval of the International Commission before it becomes effective ?

6. Language in which the establishment of neotypes should be published : Colonel R. Meinertzhagen, who attaches importance to the institution of adequate safeguards if neotypes are to be officially recognised, has added a suggestion (in Document D. 2/18 (: 129)) not previously made by any author, namely that it should be part of the scheme that no valid description of a neotype could be made in any language other than English, French, or German.

7. Relation of nominal species for which neotypes are established to other " nomina dubia " : The basis of the argument in favour of the recognition of neotypes is that, until this is done, it is not possible to recognise with certainty the taxonomic species represented by a given nominal species. In other words, the name of the nominal species for which a neotype is suggested is *nomen dubium*, though of a special kind. It belongs to that group of *nomina dubia* currently accepted, but which, in the absence of a mechanism not at present provided in the *Règles*, cannot be regarded as free from challenge. Further, within this group of *nomina dubia* there is a special sub-group where it is represented that some mechanism such as a neotype is required in order to provide for the further refinement needed when, with the advance of taxonomic knowledge, it is found that what for a hundred years or more was universally considered to be one species is actually a mixture of two or more species and where, as matters now stand, there is no means—owing to the lack of type material—of determining to which of the species concerned the name should apply. Two points arise here for consideration :—

(1) Is it not essential that the rules to be adopted if neotypes are to

be officially recognised should provide explicitly that neotypes may be established only for the purpose of removing instability and confusion arising from doubts as to the taxonomic species represented by a given nominal species in cases where the name of that nominal species is in wide use and that in no circumstances may a neotype be established for the purpose of interpreting a name not in such use ? (Then would follow the further rules, e.g., those relating to the loss, etc., of type material governing the creation of neotypes.) It is recognised that occasionally the mere existence of a *nomen dubium* may constitute a serious threat to the stability of the nomenclature of a given group, but it is suggested that the best way to remove a danger of this kind is to ask the Commission to suppress the name concerned.

(2) Would it be possible to meet the object sought by those who advocate the establishment of neotypes by a different method and one perhaps more in harmony with the principles embodied in the *Règles* as they now stand and one which might be less open to objection from the point of view of those who dislike the whole idea of recognising neotypes as an official category of type specimen ? This possibility is discussed further in paragraph 8 below.

8. A possible alternative to the recognition of neotypes as an official category of type specimen : As the *Règles* stand, the provisions in Article 25 (Proviso (*a*)), which confer availability upon generic and specific names, are quite unqualified and there is nothing in that Article which authorises later authors to act as revisers refining the definition of a nominal species afforded by the indication, definition or description published by the original author of the name. Nevertheless, the literature is full of examples where nominal species established by old authors are currently interpreted in accordance with decisions taken by authors who, acting as first revisers, have, in effect, said : " I interpret *X-us a-us* Martin, 1809, as being the species figured by Jones in 1850 as figure 1 on plate 14 of his work. (Here follows the [title of Jones's book])." Not infrequently still later authors, having access to the specimen represented as fig. 1 on Jones's plate 14, have either stated : " This specimen is the neotype of the nominal species *X-us a-us* Martin " or " In view of Jones's action in 1850 I here select this specimen, which bears the number so-and-so and is in the collection of the so-and-so Museum to be the neotype of this species." It is evident from the papers submitted to the International Commission by, for example, Stubblefield and Muir-Wood that a " restriction " of the foregoing kind without the selection of a neotype provides the information required by the taxonomist for his work. The first point which seems to me to energe here is that, if neotypes are to be permitted to become an official category, restrictions of the foregoing kind, unaccompanied by the selection of a neotype, ought expressly to be banned. The second point[31] which seems to me to emerge just as strongly as the first is the question whether the establishment officially of a neotype is necessary at all in those

[31]See Muir-Wood (H. M.) and Stubblefield (C. J.), 1951, *Bull. zool. Nomencl.* **6** : 7-17 ; *id.*, 1951, *ibid.* **6** : 18-30.

cases, which appear to be numerous, where restrictions of what I here term the Stubblefield/Muir-Wood kind are found to be sufficient for taxonomic purposes. Accordingly, it seems to me to be a matter for consideration whether it would not be advantageous—and likely also to bridge the considerable differences which at present exist as to the conditions in which neotypes, if permitted at all, should be established—to have a twofold scheme under which when types were lost, etc. :—

(1) The *Règles* would provide for (that is, give authority for) an author to define more closely the concept represented by a given nominal species by acting expressly as a first reviser (as in the imaginary case of the reviser Jones mentioned above), action so taken being binding upon subsequent workers ; this would not preclude the subsequent intervention of a later author (let us say, Smith) acting as a second reviser if through the advance of taxonomic technique this were considered necessary. The kind of case that I have in mind where a second reviser might be required would be where the first reviser defined the meaning to be attached to a given nominal species by fixing the name of that species to a given published figure and where later the existence of a third species was discovered and it was not clear to which of two possible taxonomic species the figure selected by the first reviser was referable.

(2) The *Règles* would provide further that, where workers in any given group were of the opinion that the action by a reviser acting under the powers specified in (1) above was calculated to create instability and confusion, the case should be referred to the International Commission which, after complying with rigorous provisions regarding consultation, the giving of public notice and the like should be empowered to reverse or modify the meaning attaching to a given name through the action of the reviser concerned, the power so conferred upon the Commission to include the power, where the Commission thought it desirable in the light of the views submitted to it by specialists, to specify a given specimen in some institution to constitute the unique standard of reference for the interpretation of the name of the nominal species concerned.

It will be observed that under a system of the kind described above :—

(*a*) the maximum free play would be given to individual workers, provided that their action did not, in the opinion of their fellow workers, lead to confusion and instability in the nomenclature of the group concerned ;

(*b*) neotypes as such would not be granted an official status under the *Règles* and in consequence the strong objections felt by many to the grant to individuals (as contrasted with the Commission) of the virtually unfettered right to create " neotypes " in conditions which would be binding on other authors would be met, while the main point of those who advocate such a system (namely

their objection to the intervention by the Commission in this field) would also be met.

(signed) FRANCIS HEMMING,

Secretary to the International Commission on Zoological Nomenclature.

Temporary Secretariat of the Commission,
Hotel Excelsior,
Copenhagen,
Denmark.

27th July, 1953.

CASE NO. 3

The question of the reform of the provisions in the " Règles " relating to the naming of families and lower suprageneric groups

(*continued from page* 282)

DOCUMENT 3/41

REPORT ON THE PROBLEMS INVOLVED IN THE REGULATION OF THE NAMING OF FAMILIES AND OTHER SUPRAGENERIC CATEGORIES BELOW THAT OF SUB-ORDER

By FRANCIS HEMMING, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(circulated to the Colloquium on Zoological Nomenclature on 23rd July, 1953 as Paper Z.N.(C.)11)

Material available : The Paris Congress (1948)[32] asked that an investigation into the problems involved in the regulation of the naming of Families and other suprageneric Categories below the rank of Sub-Order should be undertaken, in conjunction with interested specialists, and a Report submitted for consideration at Copenhagen in 1953. In March 1952 the International Trust for Zoological Nomenclature published a paper (*Bull. zool. Nomencl.* **7** : 61-94), in which I made a survey of the problems which appeared to call for decision and appealed to specialists to furnish statements setting out their views as to the action which it was desirable should be taken. The Trust has published the forty documents received on this subject in Part 6/9 of volume 10 of the *Bulletin of Zoological Nomenclature* (: 167-282).

2. Questions calling for decision : The questions which call for decision are set out briefly below.

(1) Shall the names of families and associated categories be subject to regulation at all ? The replies received show that there is unanimity in favour of including in the *Règles* some provisions for the regulation of the naming of families, etc., with the exception of Keen and Muller (Document 3/29).[33]

(2) Shall the principle of priority be recognised in some form in the regulation of the naming of families, etc. ? If it is granted that

[32]See 1950, *Bull. zool. Nomencl.* **4** : 138.
[33]See page 240.

there should be some provisions in the *Règles* governing the naming of families and associated groups, the next question to be considered is whether the principle of priority in some form or another is to be included in the rules. The whole nature of these rules turns upon the answer to be given to this question. For, if the principle of priority is not to be recognised, the rules would be confined to a few points only (e.g., adoption of a type genus as base of name ; the termination to be adopted, etc.), there being no provision for determining which of a number of already published family names should be used when the type genera of those nominal families are regarded by taxonomists as belonging to the same nominal family. If however the principle of priority is to be recognised in any form—for the purpose of securing that in any given set of circumstances one family name shall be the nomenclatorially correct name to the exclusion of all others—the scheme will have to contain provisions for determining the date when a family, etc., name has been established, for dealing with synonymy of family names, for preventing homonymy in family names and kindred matters. In all the documents submitted, with the exception of that by Lemche (Document 3/28)[34] the recognition of the principle of priority is advocated.

(3) Categories to be recognised : Two questions are involved in the matters of the categories belonging to the family group to be recognised in the *Règles* : (*a*) For what categories shall special terminations be provided ? (*b*) shall any recognition be given to terms proposed for intermediate categories ? As regards the first of these questions, of the specialists who have furnished statements : (i) All favour the recognition of, and the adoption of prescribed endings for superfamilies, families, subfamilies and tribes, though there is not agreement as to the endings which it is desirable should be prescribed for the first and last of these categories. (ii) Some favour the recognition of supertribes and subtribes. (iii) Bradley suggests (Documents 3/37)[35] that provision should be made for other intermediate categories such as the cohort, a course which is strongly urged also by Kiriakoff (Document 3/23)[36]. (iv) There is fairly general agreement that it is not desirable to recognise the category supergenus. (v) Bradley (Document 3/37)[37] and others suggest that it would be sufficient if those authors who wish to make use of this category were to use for it the name of the genus concerned applying to it some descriptive epithet such as " supergenus." The first of the main questions referred to above (terminations to be used for the categories recognised) is discussed in paragraph (11) below.

(4) Co-ordinate character of names for families and associated categories : There is virtual unanimity that the names for families and supregeneric categories below the sub-ordinal level should be co-ordinate with one another, that is, that, when a name based upon the name of a contained genus (the type genus) is published with some termination to denote that it is intended to be the name of one of the foregoing categories, the same name is to be deemed to have been published also for all the other categories

[34]See pages 236-237.
[35]See apge 278.
[36]See page 211.
[37]See pages 277-278.

concerned. There seems to be general agreement also that in every case the type unit of every category should be the type genus, that is, that it should not be the next lower category below the category concerned (i.e., that the type unit of a family should be a type genus and not a type-subfamily).

(5) Suggested definition of the familial category : If (see (4) above) names published for all the foregoing categories are to be co-ordinate with one another, they will all be subject to the same rules. For this purpose, as observed by Bradley, it will be convenient to devise some agreed omnibus expression which, by definition, applies to them all. A possible formula is suggested by Bradley (Document D.3/34)[38].

(6) Formation of names of units belonging to the familial group of categories : There is complete agreement that names of units belonging to the familial category should (1) be based upon the name of the type genus, to which should be added a distinctive termination. The question for decision is in what form should the name of the type genus be incorporated. That the old rule that its " root " (French " radical ") should be so taken is clearly unworkable, while even to take the " stem " (French : " thême ") is not always easy. Bradley makes a suggestion (Document D.3/34)[39], which is supported by the American Committee on Entomological Nomenclature (Document D.3/35)[40] which is designed to overcome these difficulties.

(7) Question whether any exceptions should be granted to the rule that a family name must be based upon the name of a contained genus which thus becomes the type genus : There is general agreement that the loophole providing for the exceptional recognition of a family name not based upon the name of a contained genus recognised in *Opinion* 141[41] is unnecessary because obsolete and should be withdrawn.

(8) Question whether, in order to secure availability, a family name, when first published, must be in a Latinised form : In my 1952 Survey[42] I drew attention to the fact in the early part of the last century groupings now accepted as of familial rank had been established with vernacular names based upon the names of included genera, and I suggested that, if well-known names were not to be displaced, provision would need to be made to provide such names with availability. (I had in mind especially names with French endings published—notably in entomology—at the beginning of the XIXth century). The general view seems to be that the rule should provide that, in order to be accepted, the expressions should be in Latin or in Latinised form. The view is expressed also—though in some cases reluctantly—that some special provision should be made to meet the type of case referred to above.

(9) Question whether the new rules for the formation of family, etc., names should be the same for names to be published in the future

[38]See page 258 (suggestion (c) on paragraphs 43-48 of Mr. Hemming's 1952 Survey).
[39]See page 255 (comment on paragraphs 4-6 of Mr. Hemming's 1952 Survey).
[40]See page 263.
[41]See 1943, *Ops. Decls. int. Comm. zool. Nomencl.* **2** : 55-66.
[42]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 71-72.

as for names published in the past : In my 1952 survey I suggested[43] that it might be found desirable to select some date—I tentatively suggested 1850—as the date up to which a name should be regarded as having been validly published, even if it was not then published with one of the prescribed endings. The general view seems to be against a date-line of this type. The papers which have been received do not deal with the further question whether, if the past is not to be divided into sections, the future should in addition be placed on the same footing as the past. Nevertheless, this is a question which, I suggest, merits consideration, especially if, as suggested in (8) above, a concession is to be made in favour of group terms for units of the family-name complex when published not in Latin or in Latinised form (e.g., in the case cited, in French) but as vernacular words.

(10) Proposed insertion of a Schedule containing provisions relating to the method to be adopted in forming family names from Latin and Greek words : I suggested in my 1952 Survey[44] the addition to the *Règles* of a Schedule of the above kind. The need for a provision of this sort has been recognised by all those who have commented on this point.

(11) Terminations for names of categories of the familial group : The views expressed as to the terminations which it is desired should be added to names of units belonging to the various categories of the familial group may be summarised as follows :—

(*a*) **Superfamily :** The termination " -oidea " is recommended by : (i) Kiriakoff (Document D.3/23)[45] ; (ii) the Royal Ontario Museum of Zoology and Palaeontology (Document D.3/27)[46] ; (iii) Sabrosky and (iv) Usinger (Document D.3/34, Appendix 1)[47] ; (v) Zimmer (Document D.3/34, Appendix 4, Enclosure 5)[48]. On the other hand, it is pointed out that " -acea " is commonly used in Mollusca. See Baily (Document D.3/26)[49] and Keen & Muller (Document D.3/29)[50]. The termination " -icae " is recommended by Moore (Document D.3/25)[51] for the *Treatise on Invertebrate Paleontology*. Some authors discuss the possibility of permitting different endings in different groups.

(*b*) **Family and Subfamily :** There is unanimity in favour of the terminations " -idae " and " -inae " for family and subfamily names respectively. Jeannel's suggestion (Document D.3/9)[52] of " -itae " for subfamilies is discussed by some authors but is rejected.

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[43]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 72.
[44]See Hemming, 1952, *ibid.* **7** : 66-67.
[45]See page 211.
[46]See page 233.
[47]See page 248.
[48]See page 262.
[49]See page 231.
[50]See page 240.
[51]See pages 216 and 220.
[52]See page 186.

(*c*) **Tribe :** All the authors who discuss this subject recommend the termination " -ini ", except Moore (Document D.3/25)[53], who advocates " -ides ".

(*d*) **Supertribe :** Kiriakoff (Document D.3/23)[54] advocates " -ines." Moore (Document D.3/25)[53] advocates " -ices."

(*e*) **Subtribe :** Kiriakoff (Document D.3/23)[54] advocates " -idi." Moore (Document D.3/25)[53] advocates " -ines."

(12) Date of publication of name : There is difference of opinion as to the date as from which a name for a unit of the familial group should be accepted as having been published. The majority view is that in the interest of stability the date should be the date on which such a name was so published, irrespective of whether the termination then used was one of the present prescribed terminations. The family, etc., name would be attributed to the original author (see (22) below).

(13) Selection of family names : Everyone (Townes excepted)[55] considers that an author should be free to select any genus to be the type genus of a family and that he should not be bound to select whichever of the genera placed by him in the family happens to have the oldest generic name. Similarly, there is no support for the changing of family names when, owing to subjective (taxonomic) judgments a genus is placed in an existing family which is based upon a generic name junior to its own.

(14) Situation where a family name has been based upon a generic name which is not objectively the oldest available name for the genus concerned : It has been suggested that in a case of this kind the family name might be allowed to stand or alternatively that the Commission might in suitable cases conserve it under its Plenary Powers. It is to be understood that in this case no change in the type genus of the family would be involved, the only issue being the name for the genus to be used as the basis of the family name.

(15) Situation where a family name has been based upon a genus, the type species of which is subjectively identified with a species which is the type species of another genus : Two views have been expressed : (1) that in such a case there should be no change in the family name, since the generic name is objectively an available name ; (2) that the family name should be changed in order that it may be based upon the name of the genus which is subjectively regarded as having the oldest name available for the taxonomic unit concerned.

(16) Situation arising where two or more families have been established and later their type genera are regarded on taxonomic grounds as being confamilial with one another : The question here for decision

[53]See page 220.
[54]See page 211.
[55]See page 260 (Document D. 3/34, Appendix 4, Enclosure 3).

is what family name shall be used when two (or more) genera—each forming the basis of an available family name—are treated on taxonomic grounds as belonging to a single taxonomic family. This is a straight case of subjective synonomy, it being agreed that the species which are the type species of the type genera of the families concerned are quite distinct from one another. The general view appears to be that cases of this sort should be settled by the application of the principle of priority, the relative priority of the family names concerned being determined in the manner suggested in (12) above.

(17) Situation arising when one of two or more competing family names is based upon a genus of marginal affinity and in consequence the name of a collective family is liable to constant change owing to differences of opinion on the taxonomic issue involved : The suggestion made in my 1952 Survey[56] was that means should be secured to protect a family name from constant change where there is (and is likely to continue to be) difference of opinion as to whether the genus which is the type genus of a family which happens to have been established before one or more other families is or is not confamilial with the genus (or genera) which is (or are) the type genus (or type genera) of the other families concerned. The prospect of constant instability in such cases appears to be insoluble without resort to the Plenary Powers of the Commission.

(18) Situation arising when it is necessary to change the name of the type genus of a family owing to the name of that genus being found to be a junior homonym of another generic name : This is the case presented by the family names ERYCINIDAE and RIODINIDAE discussed in my 1952 Survey[57]. Everyone agrees that, as in this case the generic name *Erycina* is an invalid junior homonym, the name of the family must be changed to RIODINIDAE, as the name *Riodina* is the name which objectively replaces the invalid name *Erycina*. There is disagreement however, as to the priority to be assigned to the name RIODINIDAE. Shall it be back-dated to the priority enjoyed by the (invalid) name ERYCINIDAE or shall it rank only from the date on which it was actually published ? In the latter event it is liable to be displaced on grounds of priority by any name of the familial group of names published between the date on which the name ERYCINIDAE was published and that on which it (RIODINIDAE) was itself published.

(19) Homonymy in family names : There is agreement that homonymy as between family names should be prevented. The general view seems to be that, other things being equal, the senior homonym should prevail over the junior one, but that, where the invalidation of the senior homonym would not cause serious disturbance in practice but the disappearance of the junior one would cause such disturbance, the Commission should validate the junior name by suppressing the senior one. It is pointed out that merely to provide that in cases of homonymy the junior family name shall become invalid would create a vacuum, for there would be no means available for providing the

[56]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 78-79.
[57]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 73-74.

family in question with a name, since the generic name on which that family name is based is not a homonym, even though the family names based upon the two generic names in question are homonyms of one another. The suggestion has been put forward that in all such cases the Commission should take action under the Plenary Powers to provide a suitably modified name for the family without change in its type genus.

(20) Rules for determining whether a condition of homonymy exists between any given pair of names in the familial group : In my 1952 Survey [58] I drew attention to the need for a rule on the above subject as part of the machinery for preventing homonymy in family names. This proposal has been supported by all who have expressly commented on it.

(21) Instability in family names arising from changes in the species accepted as the type species of their type genera : It is pointed out by Sabrosky (Document D.3/34, Appendix 1)[59] and Bradley (Document D.3/34, Appendix 2)[60] that serious disturbance at the family-name level arises when it is found that the type species of the type genus of a family is not the species thought to be such at the time when the family was founded and which has been accepted as such by later workers. The case specially referred to is that where a genus is found to have been based on a misidentified type species ; it arises however in exactly the same way when it is found that the species currently accepted as the type species of a genus is not the first to have been so selected. It is suggested by the American Committee of Entomological Nomenclature (Document D.3/35)[61] that instability at the family-name level in such cases should be prevented through an instruction to the Commission to use its Plenary Powers to vary the type species of the genus in such a way as to prevent violent disturbance in the meaning attaching to given family names. In the Hymenoptera and Lepidoptera disturbances of this kind were prevented by the Commission in 1935 on applications submitted by Bradley and myself respectively[62].

(22) Citation of the names of authors of family names : In view of the extensive and growing practice of citing the names of the authors of family names, I suggested in my 1952 Survey[63] that a provision should be inserted in the scheme in regard to this subject, analogous to that already provided in the case of generic names. Opinion on this subject is divided. The foregoing suggestion has won considerable support, but on the other hand the view has been expressed in a number of papers that the citation of authors' names is unnecessary and undesirable in the case of family names (as providing conditions for the spread of the disease " Mihi-itis "). Clearly, however, the mere omission of a provision dealing with this subject would do nothing to

[58]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 83-84.
[59]See page 254.
[60]See page 258.
[61]See page 269.
[62]See the Official Record of the Proceedings of the International Commission on Zoological Nomenclature, Lisbon Session, 1935, published in 1943, *Bull. zool. Nomencl.* **1** : 20-22, 23 (Lepidoptera cases), 27-30 (Hymenoptera cases).
[63]See Hemming, 1952, *ibid.* **7** : 82.

stem the growing tide and would lead merely to lack of uniformity. It is suggested, as a middle course, that there might be adopted a provision stating that, although it is not normally necessary to cite the name of the author of a family (etc.) name, the form of citation to be adopted where it is considered desirable to include a reference of this kind, should be as follows : . . . .

(22A) In my 1952 Survey[64] I raised also the question of the form of citation to be adopted in cases of the ERYCINIDAE/RIODINIDAE type (see (18) above) and I suggested a formula for this purpose. Various alternative formulae have since been suggested by various authors, but clearly no final view as to the form of citation required can be formed until after a decision has been taken on the critical question of back-dating (see (18) above).

(23) Emendation of family names : It has been pointed out by various authors that no question should be held to arise in regard to the emendation of family names, in so far as concerns the portion of the name which consists of part of the name of the type genus. The spelling of this portion of the name of a family should, it is suggested, follow the generic name itself.

(24) Correction of misformed family names : The scheme advocated by Bradley and others (see (6) above) contemplates an arbitrary solution for the formation of family names when consisting of barbarous words (i.e., of any word other than a Latin or Greek word). As regards family names based on Latin or Greek words the solution there recommended consists of a mandatory provision, any infringement of which would be followed by automatic correction.

(25) Question whether safeguards should be inserted in the interests of stability to prevent name-changing in the case of family-names published before the introduction of the new scheme in cases where such names consist of Latin or Greek words and are incorrectly formed : In my 1952 Survey[65] I suggested that in the interests of stability at the family-name level safeguards of this kind were desirable and indeed essential. The actual proposal there submitted is given at the top of page 69 of my Survey. Opinion on this subject is divided on rather unusual lines. There is considerable support for the idea embodied in my suggestion but some authors, without denying that the omission of safeguards will lead to name-changing at the family-name level, nevertheless recommend that no safeguards should be adopted, the authors concerned basing their view upon the desirability of keeping the rules short.

(26) Name of the subfamily, tribe, etc., which contains the genus which is the type genus of the family concerned : On this subject (which was raised in my 1952 Survey[66]) there is complete agreement that the sub-family, tribe, etc. which contain the type genus of a family should not

[64]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 82
[65]See Hemming, 1952, *ibid.* **7** : 67-70.
[66]See Hemming, 1952, *ibid.* **7** : 85.

be given a separate name but should bear the same name as the family differing from it only in termination. Several authors point out that, in view of the proposed co-ordinate character of names for all categories in the familial group, a name is potentially published for every category within a family immediately a name is published for any one of these categories. It is suggested (e.g., by Wright in Document D.3/32[67]) that in these cases no question of authorship arises and that no author's name should be cited.

(27) Proposed establishment of an " Official List " of Family Names and of an " Official Index " of Rejected and Invalid Family Names : In my 1952 Survey[68] I suggested that the above " Official List " and " Official Index " should be established and put forward various supplementary proposals in regard thereto. These proposals have secured very wide support (and also a very small amount of opposition). Bradley (Document 3/34)[69] made a suggestion as to the form of entry to be made and suggested also a change in the title, suggestions which were supported by the American Committee on Entomological Nomenclature (Document 3/35)[70].

(signed) FRANCIS HEMMING,
Secretary to the International Commission on Zoological Nomenclature.

28, Park Village East,
Regents Park,
London, N.W.1,
England.
23rd July, 1953.

[67]See page 244.
[68]See Hemming, 1952, *Bull. zool. Nomencl.* **7** : 90-93.
[69]See page 258.
[70]See page 269.

Corrigenda

page 59. Heading: substitute "ERICH" for "ERICK"

page 60. Heading: substitute "NOMENCLATURE" for "NOMEMCLATURE"

page 235. Heading: substitute "1952" for "1953"

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